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VOL. V, NO. 1 MARCH, 1930

THE QUARTERLY REVIEW
of BIOLOGY

THE "CONCEPT OF ORGANISM" AND THE RELATION BETWEEN EMBRYOLOGY AND GENETICS

PART I

BY J. H. WOODGER
University of London

IT HAS become almost a commonplace of the times in which we live to demand that knowledge—scientifically garnered and sifted—should be applied in all human undertakings. It is no longer considered sufficient, as it was in former times, to be guided by "intuition" or rule of thumb tradition, however successful this may be. No. Nowadays it is held that the process in question, whatever it is, must first be subjected to scientific analysis, and the work carried out in the light of the knowledge thus obtained. Now it seems at first sight a curious fact that the very people who are responsible for this change, or at least for providing the knowledge upon which it is based, do not apply the principle to their own activities. That is to say, the people who pursue natural scientific knowledge may be said, as a rule, and from one point of view at least, not to know what they are doing in somewhat the same sense in which a cook may be said not to know what she is doing when she uses baking powder. She knows that with this ingredient the buns will "rise," and so long as this is the case why need she concern herself with the properties of CO_2 or the laws of gases? In the same way, so long as the investigator of nature continues to make discoveries (and the volume of papers issued in the quarterly journals shows no sign of decline) there would seem to be no occasion to deflect attention from the business of investigating nature to matters relating to the process of investigation itself.

But the situation is complicated in various ways which require disentangling. In the first place the biological student does in fact receive instruction in the use of instruments, dyes, etc., based upon knowledge obtained by a scientific study of such things, and he is also required to know a great deal about other branches of natural science which will be involved in his biological investigations. But there are other important points which are

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liable to be overlooked. It is generally believed that the business of the biologist is not exhausted when he has made his observations and recorded them in the quarterly journals. Those journals are so many store-houses of data in a raw state and, in a sense, in an "unavailable" form,—in a form, that is to say, in which they are of little interest or value to anyone but the small group of specialists in the particular branch of biology concerned. Before such knowledge can become of wider availability it must be subjected to a *further* process through which it becomes articulated into a wider body of knowledge and brought into relation with other branches of the subject. Thus there appear to be two different but related processes: investigation and interpretation, and these two processes appear to be different in their nature, their outcome, and in the "canons" which regulate them. The investigatory process reduces at bottom either to observing organisms or parts of organisms in their natural relations, or to altering their natural relations in a systematic way, and recording the results. And, since the primary aim of investigation is discovery, whatever procedure leads to discovery will be accounted *good*, and whatever methods fail to achieve this end will be rejected. Thus, as far as methods of investigation are concerned (whether this refers to technical processes or to intellectual tools, i.e., "working hypotheses") heuristic success will be the touchstone by which the investigator will measure all things.

So much, then, for the criteria by which we judge the value of a method of investigation. But how do we judge whether the *outcome* of such methods is "good"? The outcome of an investigation is a series of propositions which are conveyed to the world at large by means of printed sentences in the quarterly journals, and the primary requisite of such propositions is that they should be *true*. The majority of such propositions simply state what did in fact happen in a certain laboratory on a certain date under more or less definitely known conditions. We all know what is meant by saying that such records are true, and no honest man would include false propositions in his report if he knows they are false. But when we consider the process of interpretation and *its* outcome the situation is different. Here it is a question of intellectually mastering the data furnished by the former process—of systematizing the propositions already obtained in the form of general propositions which are hypothetical in the sense that we do not *know* that they are true, although we know that they are either true or false. And we are usually in the position that it may be possible to discover whether they are false, but it will never be possible to *know* that they are true. And by knowing I *mean* knowing, *sensu stricto*, as contrasted with supposing or believing.

Now it is clear that the sort of systematized knowledge about instruments and dyes, etc., which the investigator uses to guide him in his pursuits, is natural scientific knowledge. It is applied physics and chemistry. But the sort of knowledge required for the guidance of the process of interpretation will be knowledge about the properties of *knowledge itself*, and will not be natural scientific knowledge. It seems to be the case as a general rule that the people who pursue natural scientific knowledge never pay much attention to knowledge about knowledge itself, and the people who make knowledge itself an object of scientific investigation do not always know much about the subject matter of natural scientific knowledge. It is this state of affairs which is

responsible for a situation comparable to that of the cook and the baking powder. Everyone recognizes the desirability of knowing something about the physical instruments used in scientific investigation, but the importance of understanding the properties of the intellectual tools involved—concepts, propositions, principles of inference, "working hypotheses," postulates, etc.,—is much less clearly appreciated. And from the standpoint of the process of *interpretation* this *may* be a misfortune.

An excellent illustration of the desirability of devoting some attention to these matters is furnished by the history of comparative psychology as related in a recent article in this REVIEW by C. J. Warden (11). The point of special interest in this example is that the position reached at the end of the story could have been grasped perfectly well at the beginning if more attention had been paid to an understanding of knowledge itself, and if certain speculative assumptions had not been permitted to influence empirical investigation. Such assumptions always have their "good" and "bad" sides. On the one hand they often lead people to make certain investigations which they would not otherwise have done, and hence lead to discoveries, and they are therefore important heuristically. But they are bad if they are allowed to influence the outcome of an investigation, if they lead to too great a restriction of the field of interest, and to a one-sided selection of evidence. All these points are illustrated in the paper referred to.

It is important to distinguish three kinds of assumptions. First, there are those commonly called "working hypotheses," and obviously the first requisite of such an assumption is that it *will* work, i.e. is such that it can be put to an empirical test which will definitely *disprove* it if it is false. We cannot put it more positively than that because, if the test does *not* disprove it, it does not follow that it is true. This happens, for example, in the case of hypotheses which assert the existence of entities which can never be observed. A good example of such a hypothesis which *could* be disproved is furnished by Weismann's hypothesis of development. But there are also hypotheses of a totally different character which are such that no experimental test can *possibly* be devised which would decisively disprove them if they were false. Of these there are two distinct kinds. First, there are those assumptions which, if false, would render nugatory all intellectual activity. In regard to these, although we have no *reason* for believing them to be true, we yet have a very strong *motive* for believing them to be so. In natural science we adopt certain assumptions of this kind: they are *postulates* necessary for the possibility of knowledge. The second type of untestable assumptions embraces those which are not necessary for the possibility of knowledge. These might be distinguished as *metaphysical* assumptions, and it is clear that if they are admitted into science their proper character should not be forgotten, especially if they are taken over uncritically from common-sense beliefs. Also regarding assumptions in general, it is evident that if you *assume* a certain proposition p, then any proposition which depends for its truth upon the truth of p must itself be hypothetical and cannot be regarded as one of the facts which are the outcome of your investigation. And if it is considered necessary to assume that p is true, if even the possibility of its falsehood cannot be entertained, then clearly the aim and outcome of your investigation cannot be to support or to deny p, but must be confined to propositions which

will be true only *if p* is true. It is necessary to draw attention to these points because they are involved in what follows and have often been neglected.

There is another feature of natural science in regard to which "knowledge about knowledge itself" is important. We distinguish between experimental and theoretical physics but we make no such distinction in biology. There are biological theories in abundance but no theoretical biology. There are books in plenty on experimental embryology but none on theoretical embryology. Why is this? One reason seems to be that biological knowledge is believed to progress by "observation and experiment." It is commonly taught in popular books on the history of natural science that at the Renaissance someone discovered that observation and experiment were essential to the attainment of natural knowledge, and that it was this which distinguished the later centuries from the Middle Ages. This is perfectly true, but it is a half-truth, and half-truths are usually dangerous. What is here omitted is every bit as important and essential to scientific progress as what is asserted. If physics had proceeded on these lines there would never have been any theoretical physics, and if biology does not pay attention to what is here omitted it is doubtful whether there will ever be any theoretical biology. We therefore require to discover what this other ingredient may be. Theoretical physics is sometimes called *mathematical* physics, and this seems to have led some biologists to suppose that all we have to do is to "apply" mathematics to biological data and theoretical biology will automatically emerge. This is, I think, a mistake founded upon a too simple and too superficial view of the situation. It is not so much the mathematical laws of physics as such that we require, but the mathematical *method* in the broadest sense, and this is quite a different thing. Natural scientific knowledge springs from a fertilizing union of two "realms": the realm of sense-experience or perception, on the one hand, and the "logical realm" or the realm of abstract logical entities and relations, on the other. The mistake of the Middle Ages was to neglect the former and to attend to only a very restricted aspect of the latter; the mistake of biology has been to concentrate entirely on the former and to use the latter only unconsciously or "intuitively." The success of theoretical physics has depended on the fact that, aided by pure mathematics, it has been able to explore and combine both. But it is a great mistake to suppose that mathematics as ordinarily understood exhausts the logical realm. It is now recognized that it represents only certain aspects of it, and this fact has come to light in comparatively recent years as a result of investigations into the nature of pure mathematics itself. Knowledge about knowledge itself is very important for understanding the logical realm.

II

Biology at the present day seems to be passing through a critical period in which new ideas are clamoring for attention, whilst their recognition is being retarded by various conservative factors. In various quarters signs are discernible that this is slowly becoming realized. As far as *investigation* is concerned the turn of the tide took place some time ago, when what has been called the "romantic" period came to an end with the tardy recognition that experiment was required for further progress. But accompanying these changes in the standpoint of investigators there has been no fundamental change in biological *interpretation*. It is here that

change is confronted with the most formidable obstacles. It is easy enough to appreciate the need for supplementing observation by experiment, but it is quite another thing to foresee the need for a revision of our ways of thinking, and a very difficult thing indeed to carry out such a revision. These things appear to be better understood in Germany than in the English-speaking countries. In England it is not among men of science that we find most interest taken in the newer biological ideas. But in 1919 J. Schaxel (10) published a book in which he pointed out the exceedingly heterogeneous nature of biological thought, drew attention to its inconsistencies, and urged the need for a critical sifting of its fundamental concepts. The first edition was out of print before the end of the year and a second was issued in 1922. It is difficult to imagine such a book passing through two editions in England. Schaxel's chief aim was to drive home the necessity of understanding that strictness of *thought*, as well as exactness of investigation, is essential if biology is to emerge from the morass into which its "careless *Begriffsromantik*" (as Schaxel calls it) has brought it. Since the first publication of this book Schaxel has edited a series of *Abhandlungen zur theoretischen Biologie*, to which a number of German thinkers have contributed, and it is among these that the reader may discern the signs of reawakening to which reference has been made. These writers all recognize that a given set of assumptions will commit you to a certain definite set of conclusions. But in biology our fundamental notions are for the most part so vague and lacking in precise definition that it is impossible to work out their logical consequences to any clear cut result, as any one will have realized who has attempted to discover the precise points at issue in the traditional biological controversies. The German authors make a distinction between "law" (Gesetz) and "rule" (Regel) which recognizes the distinction between what I have called the logical and empirical realms. By a *law* they mean an *a priori* logical system capable of strict deductive development, although this need not be "mathematical" in the narrow sense, but may be "logistic" in the sense of C. I. Lewis (9). By a *rule* these authors simply mean an empirical inductive generalization. But by no means all German biological writers either make this distinction in the above way or realise its importance. Thus B. Dürken (4) writes:

> It must never be forgotten that the task of the exact investigation of nature does not consist in trying to explain nature by deduction from general principles, and so adapting the facts to preconceived opinions; but lies in the attempt to obtain a general view of natural occurrences inductively from the greatest possible number of isolated facts.

Now this is certainly not the way in which physics developed, as illustrated, for example, by the procedure of Galileo and Kepler. It represents only one aspect of the process. Induction is necessary but not sufficient. It will give us biological theories but not theoretical biology. Only if and when a system of logical relations is discovered from which the empirical generalizations can be deductively developed and into which the biological concepts enter as values of the logical variables will anything approaching theoretical biology, which is at all comparable with theoretical physics, be possible.

The authors in Schaxel's *Abhandlungen* deal, for the most part, with the clarification of biological concepts, a most necessary preliminary, and also with the working out of new ones. And if it is possible to pick out one theme which is especially prominent in these discus-

sions it seems to be one which turns on the question whether we are to regard the organism *as* an organism or not, and on the difficulties which present themselves when we answer this question in the affirmative. To an outsider it will seem strange that there should be any such question, but it admits of course of a simple historical explanation. In the past the concept of organism has not been employed by the majority of biologists, but instead, owing to the methodological success of the notions bequeathed to us by René Descartes, organisms have been more commonly regarded as, in some sense, machines, although in what sense has not always been clear. I have attempted to clarify this question elsewhere (17). To discover what the "concept of organism" means at the present day one must consult, not a biologist, but certain types of philosopher, or, curiously enough, certain types of mathematical physicist. In histories of biology in the dim future there will probably be a chapter entitled "The Struggle for Existence of the Concept of Organism in the Early Twentieth Century," which will relate how this concept came to be neglected on account of the influence of Descartes, how the metaphysics of natural science in the Nineteenth Century so completely dazzled biologists that they never dreamed of regarding organisms as being anything but swarms of little invisible hard lumps in motion, and how the first blossoming of the concept of organism towards the end of the century was nipped in the bud by the mismanagement of those who advocated it. The rest of the chapter remains to be written. It is of no use dogmatizing or getting emotional about it.

The story is tolerably simple when it is disengaged from the learned verbiage in which it is apt to be concealed. Descartes, who invented the machine theory, understood perfectly well that a machine presupposes a mechanic, but where was the mechanic of the living machine? Descartes was a pious man, and was in the habit of appealing to God to get him out of difficulties. It was to God that he appealed to overcome the difficulty of discriminating between dreams and waking experience, and thus to reassure himself of the existence of an external world, after the celebrated method of doubt had been pushed too far. It was to God that Descartes appealed to furnish the missing mechanic for the organic machines. That was in the days when "development" meant "evolutio." Thus Descartes set the fashion of regarding the organism as a machine with a transcendent mechanic or "organizing principle" and this practice has been followed by some biologists ever since. But others—and they form the majority—who had no use for transcendent principles, and who lacked the Cartesian consistency, have contrived to get on with a machine *without* a mechanic, and this, as Euclid says, is absurd. But nevertheless this view *worked*: that was the main thing, although it did not alter the fact that it was absurd. Only a few thoughtful people have seen that it is absurd. To Driesch belongs the credit of seeing this more clearly than anyone else and of keeping people constantly uneasy about it. But instead of throwing over the machine theory he retains it with a transcendent mechanic, although not in the Cartesian manner. This naturally so disgusted the rank and file that they failed to give sufficient attention to Driesch's admirable critical work. Thus the concept of organism failed to receive due attention because Driesch's mechanic was of no help from the scientific standpoint, however successful it may have been from the metaphysi-

cal. But nevertheless some of Driesch's arguments are important and we cannot refute them merely by expressing our dislike of their supposed consequences.

In England the story has taken a different course. Here the shortcomings of the machine theory have been appreciated by a physiologist, J. S. Haldane. He has perceived not only the contradictions of the machine theory but also the difficulties of appealing to a transcendent mechanic. Accordingly he has urged the abandonment of the machine theory and the desirability of treating the organism *as* an organism. Why is it then that he has had just as small a following as Driesch? The chief reason seems to be that J. S. Haldane has been content to use the "concept of organism" intuitively in his own work without attempting to give it an abstract formulation. His critical arguments are not so compelling as those of Driesch for a similar reason, and his assertions regarding what he wishes to put in the place of the machine theory are somewhat vague, and require "sympathetic" interpretation. Both of these eminent writers seem to have shown a deplorable lack of understanding of the psychology of the scientific investigator. Moreover, obstacles have also been created by the horrible ambiguity attaching to the term "mechanistic." Abandoning the machine theory does not necessarily mean abandoning "mechanism" in *all* its various meanings, as J. S. Haldane seems to suppose. Neither is "physical" synonymous with "mechanical." It is important to understand this. But Haldane has had the depth of vision to see that far more than a mere re-shuffling or re-definition of biological concepts is involved in the reform of biological thought. It requires investigations which go below the specifically biological level into problems concerning "knowledge about knowledge itself," and it is the difficulties attendant upon an intellectual upheaval of this kind which are largely responsible for the backwardness of the revolution in biological thinking. We do not always appreciate the harm done by the camp followers of great men, in whose hands discoveries which were at first fluid and tentative become petrified into dogmas which create obstacles to further progress. In this way the logical dogmas of Aristotle, the psychological dogmas of Locke, the biological dogmas of Darwin, and above all, the physical and metaphysical dogmas of Descartes are all operative to-day even in the biological sciences. Such dogmas may constitute intellectual blind-spots preventing us from realizing to what a great extent we are free to explore new ways of thinking if new empirical data should require them. But as tentative assumptions harden into dogmas which are never examined our thoughts become encrusted with layers of intellectual rubbish which require the labors of an intellectual Hercules for their proper purgation. Among modern writers who have felt the need for such an undertaking few are better equipped than A. N. Whitehead, and the reader who wishes to learn what it involves, and why it is necessary, cannot do better than read the splendid first two chapters of that author's *Concept of Nature* (14). I have discussed some of these problems from the biological standpoint in a recent publication (18).

Perhaps we can apply G. K. Chesterton's remark about Christianity to the "concept of organism": "It has not been tried and found wanting; it has been found difficult and not tried."

III

After these necessary preliminaries I turn now to the chief task of this paper.

I wish first to introduce and define clearly some concepts which appear to be involved in the "concept of organism," to analyse a type of logical order which appears to be exemplified in the organic realm, to examine the use of the causal postulate in biology, and then to apply all this to certain difficulties in embryology and genetics in order to trace them to their sources, and to enable us to think clearly about them. It is hopeless to work in a fog from lack of proper analysis of your thought, and it is equally hopeless to pursue a policy of obscurantism which refuses to allow awkward questions to be raised and openly examined. The situation is complicated and its difficulties are enhanced by the impossibility of saying everything at once. It will be necessary to explain first a number of seemingly disconnected notions and then to try to bring them together at the end.

The concept of organism requires a number of subsidiary notions such as "organic whole," "organic part" and "organic relation." Also an organism exhibits what I shall call "hierarchical order." It is easy enough to see "intuitively" what is meant by these terms; there has been a good deal of vague talk from time to time about "the whole being more than the sum of its parts," etc.; the difficulty is to make these notions precise in order to enable us to see how we can use them for scientific purposes. Intuition is the indispensable cutting edge of intellectual inquiry, but the ground won is not consolidated until it has passed from the stage of intuitive apprehension to that of logical analysis.

I shall first try to state quite abstractly what is intended by "hierarchical order," and by abstractly I mean without reference to any *particular* exemplification. We have to investigate hierarchical order as it is in the "logical realm" in its most generalized form. Only then can we profitably discuss its empirical exemplifications. Any reader who finds difficulty in following the abstract exposition can easily make for himself a "particular exemplification" by simply drawing a square on a piece of paper, dividing it into four quarter squares, then dividing each of these into four and so on *ad libitum*. But it must be remembered that this *is* only a particular exemplification, and that hierarchical order has, as such, nothing to do with "space."

The notion of order requires "individuals" or single entities for thought, classes of such, and relations. In hierarchical order we begin with a single individual which will be symbolized by W. This is analysable into individuals called members (m or M) which fall into classes of two kinds called *levels* (L), and *assemblages* (A). There is also a fundamental relation (R_H) in which the members stand to one another, and upon which the whole hierarchical type of order depends. (This will be the relation "being a quarter of" in the case of the square.) We proceed to the following definitions:

Level: A level is a class of members of W which exhaustively divides W and is such that no member of the class stands in the relation R_H to any other member of the class. In any hierarchy there are at least two levels.

Highest level: One level is such that none of its members stand in the relation R_H to any other member, but each stands in the relation R_H to W. This may be called the highest level.

Lowest level: if there is any level which is such that its members are incapable of further analysis this constitutes the lowest level.

Next highest level: if m is any member of a given level L then there is one and only

one level (other than L) containing one and only one member M such that m stands in the relation R_H to M. This is the next highest level above L.

Assemblage: if we take any member M belonging to any level except the lowest it will be analysable into a class of members $m_1, m_2, m_3, \ldots m_n$, which is such that it exhausts the member M from which we begin, all its members stand in the relation R_H to M, and none of them stand in the relation R_H to any other member of W. Such a class obtained by analysing any member is called an assemblage.

Thus each member of a given level L is analysable into an assemblage A of members each standing in the relation R_H to A, and since (by definition) none of the members of A stand in the relation R_H to any other member, they constitute, with the members into which the other members of the level L are analysable, a new level, which is the *level next below* L. In this way we can proceed until we reach the lowest level.

It will be found that a given member m cannot be a member of W without standing in the relation R_H to some member M belonging to the level next above its own level unless m belongs to the highest level.

In addition to the generating relation R_H there will also be relations (R_L) between members of a level, and relations (R_A) between members of an assemblage.

It is important to note that a given member can always be regarded from three points of view: (1) from the point of view of its membership of a level, i.e., its R_L relations; (2) from the point of view of its entering, with other members of its level, into the constitution of a member of the next highest level, i.e., its R_H and R_A relations; and (3) from the point of view of its analysis into an assemblage of members of the next lowest level in their R_A relations.

This brief account by no means exhausts the subtleties of hierarchical order but will suffice for present requirements. We can turn now to see how it is exemplified in nature. I follow the view of A. N. Whitehead that nature is primarily analysable into spatio-temporal entities (which he calls "events") which pass, and their characters (his "objects") which endure, i.e., can "be again." If we apply hierarchical order to spatio-temporal entities, then W will be a spatio-temporal *whole*, analysable into members which can properly be called *parts*. Moreover hierarchical order may be generated in nature through a temporal process, and this may happen in two ways: (1) by the coming together into R_H relations of originally separate entities which thus become members of a hierarchy constituting a new whole; or, (2) by the division of an original single whole into parts standing in R_H relations. The first case is exemplified in crystallization, the second in the process of development of a metazoan organism. If by a "cell" you mean a spatio-temporal entity which is such that in visual perception it is characterized by an "object" or "pattern" roughly indicated as analysable into a nucleus and cytoplasm, then it is easy to see that in a metazoan animal the parts recognizable as cells constitute a level which is homogeneous with respect to the type of organization of its members. But there may be, and usually are, parts constituting a higher level, namely parts analysable into cells, and these will be cell-assemblages or *cellular parts*. Also each cell is analysable into an assemblage of *cell-parts*, and these are ultimately analysable into the entities with which chemists deal. In cases where it is important to bear in mind that we are speaking of parts it will be desirable to refer to cells as "non-cellular parts" (following the precedent

of those who speak of protozoa as non-cellular), since the cell-type of organization characterizes wholes (e.g., germ-cells and protozoa) as well as parts, and from the organic standpoint it is important not to confuse parts and wholes since they have very different properties. Thus we have three levels recognizable in perception: (1) that of which the members are cellular parts; (2) that of which the members are non-cellular parts (cells); and (3) that of which the members are cell-parts. Below this there may of course be any number of levels not accessible to perception, and about which we can only assert hypothetical propositions. It is interesting to note that in a metazoan animal there may be spatial parts which lie outside the organic hierarchy, since they do not stand in an R_H relation to other parts, and parts do not stand in such relations to them, for example, the matrix of cartilage, and such parts are always "dead."

So far we have merely noted some obvious points about hierarchical order in the spatial organism. But this is an abstraction resulting from the separation of space from time, and there is no such thing as a living organism in abstraction from time. The living organism is always a four-dimensional entity with temporal as well as spatial extension. The purely spatial three-dimensional organism is the organism considered at a moment, and a moment is an "ideal" to which we approximate by a progressive diminution of the time-dimension in accordance with the method of extensive abstraction, as Whitehead (15) has shown. If now we conceive the organic realm from the four-dimensional standpoint we shall see other ways in which hierarchical order is exemplified. It is generated through the process of reproduction, by which an organic race comes into being. If we consider this four-dimensionally we see that if we begin with a single organism (or pair of organisms) a hierarchy is generated in which the original starting point constitutes the highest level, the F_1 generation constitutes the next level and so on. The relation R_H is the relation "being an immediate descendent of," and the assemblages in each level except the first two will be "families," i.e., the immediate descendents of a given member (or pair) of the level immediately above the one in question. Such a hierarchy would be a genetic hierarchy.

Cell-division, as already mentioned, also generates a hierarchy. Considering this from the four-dimensional standpoint we see that starting from a single cell we obtain a hierarchy in which the highest level is this starting point, and each assemblage contains only two members (when division is binary). The R_H relation will be "being an immediate cell-descendant of." The second level will contain one member (an assemblage of two cells), the next two, and so on. If the cells always divide simultaneously those belonging to the same level will always lie in the same "moment." Now there are two possibilities: (1) either the members characterized as cells are single individual organisms as in the protozoa, or (2) they remain in a certain relation to one another so as to constitute not wholes but parts of one organic whole; we then have the "spatial" hierarchy generated, as already mentioned, in the process of development in metazoa.

When a natural entity is divided there are always two possibilities: (1) the two parts may be equal in the sense of having precisely the same properties; or (2) they may be unequal and have different properties. If the process of division in the protozoa is equal in the above sense the individuals in any level will differ from

one another only in consequence of the different environmental contingencies they may encounter. In the case of the metazoa, where a single organic whole with its parts in certain determinate hierarchical relations results from division, we again have the two possibilities of equal or unequal division. If the divisions are always equal (in the sense defined above) in this case we could only, it seems, interpret any differences that might later manifest themselves between the cells by supposing that it resulted from differences in their mutual relations in the organic "spatial" hierarchy. When we are considering the metazoa there are two interpenetrating hierarchies involved: (1) that generated by the process of cell-division, call it the "division hierarchy"; and (2) that resulting from the fact that the members of this hierarchy are related by yet another relation in addition to the one which constitutes the R_H relation in the division hierarchy. This relation is that complex multiple relation between the parts in virtue of which they *are* parts and not independent entities. This seems to be what people mean by the "organic relation," and is simply the relation of "being a part in an organic spatial hierarchy". A given individual also occupies a determinate position in a genetic hierarchy.

"Cells" can be classified in various ways, depending on the kinds of difference between them. First there is the kind of difference between say, a smooth muscle cell of a monkey, and a smooth muscle cell of a man, and secondly, there is the kind of difference between a smooth muscle cell of a man and a gland cell of a man. The first difference is one which does not "develop" but merely persists. It may be called a genetic difference, and it is this mode of classification of cells which is of interest to genetics. But genetics as an *experimental* science cannot explain how the monkey-cells came to be different from the man-cells, since this was accomplished through a process which occurred in the the remote past. It can only investigate such differences as they are now, not how they came to be. But the second kind of difference is one which develops and is the concern of the embryologist. For there was a time in the history of a given man when he had no smooth muscle cells and no gland cells. From the embryologist's standpoint, therefore, we can classify cells according to the determinate place they occupy in the division hierarchy. If we follow this method we see that the cells belonging to the highest levels are very similar to one another, and we call them embryonic cells, whereas the cells belonging to the lower levels, which are also later and belong to later temporal slices of the individual history, are very different from one another. The cells of these lowest or later levels appear to be divisible into three classes: (1) tissue cells; (2) cells which we may call "persistent embryonic cells," which are concerned in restitutive processes; and (3) germ-cells. Now tissue cells are distinguished from other cells by possessing cell-parts which have *never before been present* in higher or earlier levels of the particular hierarchy in question, although they will usually have been present in previous members of the *genetic* hierarchy. Thus one of the problems of embryology seems to be to explain how these tissue-cell-parts (e.g., myofibrillae, haemoglobin, secretion-granules, etc.,) come to be developed in the cells, how it is that certain parts appear in some cells, and others in other cells, and how it is that they appear *when* they do.

Something must be said at this point about the term "differentiation." Under this term two quite distinct processes

appear to have been confused. By *differentiation* I shall mean simply a process through which two (or more) different parts come into being by the separation unequally between them of something previously present in *one* part (or whole). For example, it is by a process of differentiation in this definite but restricted sense that the cells of the animal pole of a frog's egg come to be different from those of the vegetative pole with respect to their yolk content. But there is another process by which parts may come to be different, namely when parts appear which were not previously present as such at all. Of this we have an example in the process already mentioned whereby cell-parts not previously present make their appearance in cells. I shall call such processes processes of *elaboration*, not differentiation.

It does not seem to me to be correct to define the "prospective potency" of a cell in the way Driesch (3) does as "the *possible* fate of a certain cell, i.e., the totality of possible characters of the adult into which this cell may develop." Because, in the first place, as will be seen below, cells do not develop into characters *at all*, and in the second place because only embryonic cells when they undergo their histological elaboration relatively late in development *can* be said to *develop*, and then it is actually only certain cell-parts that develop. Cells of earlier levels of the division hierarchy do not develop at all, they merely *divide*; that, and that alone, is their "fate".

We have so far considered only two embryological processes: cell-division, or the elaboration of non-cellular parts, and histological elaboration, or the elaboration of cell-parts. That the former is a process of elaboration becomes clear when we recall that the fertilized ovum has no parts characterized as cells, but only cell-parts, and that after cleavage there is a new level in the spatial hierarchy constituted of cells as parts standing in certain determinate relations to one another. It seems clear from modern genetical studies that the cells belonging to a given race will only be capable of a certain restricted number of cell-part elaborations and it is not at all difficult to understand that an important rôle in this latter process will be played by the hypothetical "genes" required by geneticists, and regarded by them as *nuclear* cell-parts. These of course do not "develop" but only persist, except in so far as mutation occurs. It would be more correct to say that their *properties* persist. If all *nuclear* divisions are equal (in the sense defined) then, in so far as histological or cell-part elaborations depend upon the nucleus, *every* non-cellular part (in a suitable temporal part of the division hierarchy) will theoretically be capable of undergoing any of the histological elaborations of which the race is capable, unless the latter is heterozygous, in which case the cells of a given individual will be capable of only certain of the possible histological elaborations.

But, at least in the higher metazoa, there is another process of elaboration not yet considered, namely the elaboration of *cellular* parts. It will be seen below that it is this developmental process which presents perhaps the greatest difficulties. But before we can proceed further in this direction we must consider the causal postulate a little, and also devote some attention to the different kinds of "properties" and how they may be affected by the kinds of relations met with in organisms. This is of the greatest importance for the interpretation of the data of modern experimental embryology.

IV

The incautious use of the notion of causation, in accordance with the wholly uncritical practice of common sense, has

led to some appalling muddles in the discussion of genetical and embryological topics. Some biologists still seem to believe that a "character" can be "solely due to" or "caused by" either a "gene" or "genetic factor" alone, or by an "environmental stimulus" alone, as though "cause" and "effect" were isolated entities standing in a two-termed relation to one another and were completely indifferent to anything else in the world. A very little reflection should suffice to dispel this illusion. Others make some improvement upon this by saying that a given character is partly dependent upon genetic factors, and partly upon environmental factors. But this too cannot be correct if by genetic factors is meant parts of chromosomes, because no organism consists simply of a mass of chromosomes, and you cannot ignore the rest of the organism without further ado. The notions of "stimulus" and "response" are also often used very loosely in this connexion with unfortunate consequences. Among Schaxel's *Abhandlungen* a useful discussion of the use of the notion of stimulus has been contributed by P. Jensen (7). The following remarks about the causal postulate are chiefly confined to the requirements of the present paper.

In any causal investigation we always seem to have two "occasions" or "situations" which are *compared*. An occasion contains three principal constituents: (1) an organism, or part of an organism; (2) an environment; and (3) an observer. Or, we may have only one occasion containing two organisms in the same environment. Some simple examples will be helpful. Consider an occasion containing an organism, an observer and some stimulus, i.e., some environmental change. Suppose the organism gives the response r. On a later occasion containing a later temporal part of the same organism the same stimulus is presented. If the same response is observed we say, in accordance with the causal postulate, that the organism has not changed (with respect to the process in question) since the first occasion. If we note a different response r' we say the organism *has* changed, it is a different organism, and it is because of this difference that the response r' differs from r, although this difference may not be observed but is hypothetical. Suppose now we take a number of fertilized eggs from a certain fish and divide them into two batches: one batch (a) is placed in normal sea-water, the other (b) is placed in water containing some abnormal constituent. If the embryos of (b) all exhibit some definite difference (the same in each embryo) from those in batch (a) we say that this difference is "due to" the abnormal constituent of the environment of (b), or, more correctly, it is causally correlated with the difference between the two environments. If we say that the abnormal constituent *caused* the observed abnormality all we are entitled to mean is that it was one element in a complex which is essential to the result observed. Finally, suppose we have two rabbits—one white and one black—born in one litter. Then we say that this difference was correlated either with a difference between the intrauterine conditions of the two embryos, or (which would be considered more probable) with some difference which was present *throughout* the development of both, i.e., a genetic difference.

Thus in all three cases we have comparison. If we observe a *difference* between the organisms concerned we assume either that the observed difference is correlated with some environmental *difference*, or that it is correlated with a previously existing, but perhaps unobserved, *difference* between the organisms concerned. We always believe that if any change occurs this is because something has happened in the

environment, or because a change has been going on unobserved in the organism. We can express all this in a simple symbolism:

(1) $D(A, B).C.D(O_A, O_B)$ or $D(E_A, E_B)$
(2) $D(A, B).C.D(O_A, O_B)$ and $D(E_A, E_B)$

These formulae are sufficiently obvious. "D" simply means "the difference between" whatever follows in brackets, and "C" means "is causally correlated with" what follows it. "*A*" and "*B*" are the two occasions (or two organisms in one occasion). "*O*" stands for organism and "*E*" for environment. We also require a third formula for cases in which the *relations* are different in the two occasions:

(3) $D(A, B).C.D(R_A, R_B)$

Suppose in two occasions I have a lighted candle and a piece of sealing wax. In "*A*" these two constituents are six feet apart, in "*B*" the wax is in the flame. In the former case the wax is solid, in the latter it is melted. The other correlated difference is then the difference between the relations of the two constituents in the two occasions. The same would hold in embryological situations in which so-called "totipotent parts" are transplanted to different situations. It may be noted in passing that because we find a second pair it does not follow that this *is* causally correlated with the first.

We must turn now to properties. I shall use the term in a wide sense to include both perceptible characters, e.g., coat color, which may characterize an organism during a considerable period of its history, and also for a specific change which an organism (or part) may exhibit when it enters as a term in a specific causal relation. The most important point which requires explanation from the standpoint of the "concept of organism" is the difference between what will be called "intrinsic" and "relational" properties. In natural science all causal properties are relational properties in the wide sense, but in considering organic wholes or parts it is necessary to distinguish between *viable* relations, and *lethal* relations, i.e., between those in which the organism (or part) lives and those in which it dies. Now by *intrinsic* properties I mean those which an organism or part exhibits in *all viable* relations, and by *relational* properties I mean those properties a *part* exhibits only in certain *organic* relations. And among intrinsic properties it will be necessary to distinguish between *original* and *acquired* intrinsic properties. Thus in a certain relation a part may acquire a property which persists even out of the relation. This would be an acquired property—acquired relationally but becoming intrinsic. This can be illustrated by analogy with "members" of a "social hierarchy." Consider an Englishman born in England. He will have certain intrinsic properties which he exhibits in all viable relations in common with all men, e.g. breathing, eating, etc. But he will also acquire certain relational properties which depend on the "specific hierarchy" to which he belongs. He will, for example, acquire the relational property of speaking English, but if in early childhood he had been "transplanted" to another "social hierarchy," e.g., to Germany, he would have acquired the different relational property of speaking German. He may also take to the sea and acquire the relational property of being steward on board ship. If he is cast upon a desert island he loses this and other relational properties and reverts to a more generalized type in which perhaps he exhibits chiefly his original intrinsic properties. Now precisely the same thing is true of the members of the various levels

in the organic hierarchy constituted by the single individual organism. Robinson Crusoe on his island is parallelled by the isolated cell in tissue culture. The important point is that the relational properties of parts depend upon their specific relations in the hierarchy to which they belong.

Now it may be the case that *all* specific histological elaborations are relational properties in the above sense, but that all the cells belonging to a given homozygous race have identical original *intrinsic* properties. But we must pause to consider certain complications. The intrinsic properties of a cell may depend upon both its cytoplasm *and* its nucleus. If two cells, which are assumed to have "equal" nuclei (in the sense defined) behave differently in the same environment, we should say (in accordance with the causal postulate) that they differed intrinsically in their cytoplasm, since their relations are supposed to be the same. But that intrinsic cytoplasmic difference may have been acquired in consequence of relational differences during development, and would therefore be an acquired relational property. But since it now persists in spite of changed relations (since by hypothesis both cells are in the same environment now) we should have to call it an acquired intrinsic property. We seem to have an example of this in the cardiac muscle fibres. These exhibit the elaboration of the rhythmical contractile property in the course of development and this (on the hypothesis under consideration) will be a relational property. But it may persist for years in tissue culture. Hence it is an acquired intrinsic property which is not lost in viable relations even if the part is removed from its place in the individual hierarchy.

Thus it is possible to conceive that throughout the four-dimensional division hierarchy the nuclei are "equal" (i.e., that all nuclear divisions are purely quantitative, except of course the meiotic ones) and that differences between cells (as far as tissue-cells are concerned) are always a consequence of acquired relational properties (which may become intrinsic). Differences between cells may arise either by differentiation or elaboration, and it is in connection with the latter process that relations and relational differences are so important. If we make these assumptions (which seem to have a good deal of empirical evidence in their favor) we shall be able to interpret and state in precise terms many of the curious results which have emerged from transplantation experiments. Some such view as this seems to me to be forced upon us. It will be seen, after a little reflection, that relational properties are of immense importance in an organism, and that far too little attention has been paid to them. It is here that the machine analogy has put us on a wrong scent. There is not discernible in machines a level constituted of parts which are such that the level is homogeneous with respect to the type of organization of those parts, as is the case with the cell-level, and in which the parts can become different in accordance with their relations in the hierarchy. Moreover the *maintenance* as well as the elaboration of specific cell-parts is in some cases dependent on their relations, as is clearly seen in some tissue-culture experiments. In other words the parts in an organic hierarchy are internally related, whereas in a machine these relations are external or non-constitutive relations. How long are we to persist in refusing to look sheer hard facts in the face, merely in the interests of a seventeeth-century analogy which by now may well have outgrown its usefulness? Sooner or later biology will have to take account of them

if there is to *be* any theoretical biology, as contrasted with a "medley of *ad hoc* hypotheses."

Another important point came to light in our consideration of hierarchical order in the abstract. It was noted that a given member can be regarded from three points of view, namely from that of (1) its membership of a level, i.e. its R_L relations; (2) its entering into the constitution of a member of the next highest level, i.e., its R_H and R_A relations; and (3) its analysis into an assemblage of members of a lower level in their R_A relations. Now it has been the custom to regard a given organic part almost exclusively from the third point of view, namely from that of its analysis into parts and their properties. But clearly in an organic hierarchy R_H relations will be of equal importance, otherwise its parts would not be different in isolation from what they are in their place in the hierarchy. In other words they would be externally related to one another, like the parts of a machine, which does not seem to be the case. It is for this reason that E. B. Wilson seems to me to be in error when he writes (16), regarding the "organization" of the germ-cells which he says "determines" the "particular course" of development:

> Nevertheless the only available path towards its exploration lies in the mechanistic assumption that somehow the organization of the germ-cell must be traceable to the physico-chemical properties of its component substances and the specific configurations which they may assume.

How will this be the case if the fertilized ovum is an organic hierarchy with levels *above* the chemical level? We should expect the properties of the members of the chemical levels to depend upon their R_H relations to members of *higher* levels. In a recent lecture in London on the results of micro-injection experiments it was stated that proteins are not present as such in certain living cells. If this is the case it is precisely what we should expect from the organic standpoint. Also it always seems to me to be dangerous to use the word *must* in natural science. That is an expression which may safely be left to fundamentalists and their friends. Let us set them a good example by saying "may" instead of "must," and "probable" instead of "certain," which is all we are usually entitled to say.

Another instance of the neglect of the implications of the concept of organism in the past is furnished by a recent paper by J. Gray (5), in which he discusses the methodological problems involved in the interpretation of growth curves. In his summary he writes:

> Graphic treatment of the data underlying a typical growth curve is liable to produce errors of considerable magnitude, and often tends to confuse the facts. The units which compose a metazoon's body form a very heterogeneous system, in which the rate of growth of one organ is dependent on that of others. It is, therefore, intrinsically improbable that the behavior of such a system should conform to that of a simple chemical system in which the variables are few in number and capable of accurate analysis. The conception of growth as a simple physico-chemical process should not be accepted in the absence of a very rigid and direct proof; at present, it rests on the results of a process of graphic analysis which is often, if not always, of a relatively inaccurate nature.

This clearly represents the first gleams of the dawn of the concept of organism in one consciousness, but a little reflection would have given us even more than this without huge chunks of algebra and pages of graphs. Who but a very learned man would dream of conceiving the growth of a metazoon as a "simple physico-chemical process"? And yet biologists no more ask for a "proof" of this than fundamentalists ask for a proof of the infallibility of the Scriptures. Surely this is a shining example of what C. D. Broad (2) calls a "silly theory" in the sense of one "which

may be held at the time one is writing professionally, but which only an inmate of a lunatic asylum would think of carrying into daily life." Now such silly theories are quite indispensable for scientific progress, but it is not only fatal to carry them into daily life (as people frequently do), it is also fatal to take them too seriously in science, lest they harden into dogmas which obscure our intellectual vision, as this one seems to have done. The whole difficulty in relation to these problems rests very largely, I think, on the deep-seated belief that only the ultimate scientific objects into which an organism is believed to be analysable are "really real" (e.g., electrons) and that everything else is "mere appearance" and can be safely neglected. But this is a big subject and here I can only refer the reader to the book by Whitehead (14) already mentioned. This too is a metaphysical legacy from an earlier age.

V

Returning now to the particular problems of genetics and embryology we first have to ask: What does a geneticist mean by a "character"? Consider, for example, a patch of black on some animal's skin: does he mean by the character the very black patch which we see, or does he perhaps mean a vast number of little pigment granules in the skin? If the latter is meant then he is not talking about a character at all but about certain *parts*, and these parts may differ in their properties (characters) from corresponding parts in a related animal. If you try to avoid this by saying that these granules are analysable into sub-parts which differ only in their relations you still do not escape from the antithesis between parts and their characters or properties. Because your ultimate parts will not be *mere* parts, i.e., spatio-temporal entities, but will be distinguished in some way from other spatio-temporal entities by characters or properties of some sort, otherwise the world would consist of a single uniform spatio-temporal entity with no "things" in it at all. Let us say, then, that *correlated* with the black patch which we see there are also, in certain cells of the skin, little pigment granules which differ in their properties from those in the skin-cells of a related animal which has, say, a *yellow* patch correlated with its pigment granules. Let us try to conceive how this is to be interpreted embryologically and genetically. We go back to the fertilized ova from which the two hierarchies were generated by division. In accordance with the chromosome hypothesis we assume that the chromosomes of these two ova differ from one another in some respect, and that with this the difference between the properties of the two kinds of skin-cell granules is correlated. Cleavage now begins, and we assume that the chromosomes are divided equally at each division. This continues until we have a quantity of cells *all* of which, so far as the nucleus is concerned, are capable of elaborating a certain sort (but only *one* sort in *each* whole embryo, i.e., according to the "genetic" classification of its cells) of pigment granule. But towards the end of development, when these granules begin to make their appearance, they do not *appear* in *all* the cells but only in *some* cells. Consequently (if we are to apply the causal postulate) we shall have to appeal to something else to "make the difference" (as we say in everyday life when we are not being "silly"). We seem to have but two alternatives (or a combination of both): either it is because the *relations* of the skin cells are different from those of the other non-cellular parts (in accordance with our third causal formula); or because the

cytoplasm is different in these cells. There are facts which would justify the assumption that *if* the difference is to be traceable to cytoplasmic differences these can themselves only be relational (not original), i.e., acquired through relational differences earlier in ontogeny. Thus the hypothetical animal we are considering might be one of those in which, at a suitable stage, it would be possible to transplant cells from, say, the black to the yellow specimen, and they might then develop, not into skin cells but into neural-tube cells with no pigment granules in them at all. As we are trying to consider a purely hypothetical general case we shall make the assumption that the differences between the pigment cells and the remaining cells of the body are *not* dependent upon original cytoplasmic differences but only on relational differences (including differences resulting perhaps from relations of earlier periods). In other words the property of developing pigment granules (as contrasted say with myofibrillae) is a *relational* property. But the property of developing "black" pigment granules in the one case, and "yellow" ones in the other, is clearly an original *intrinsic* property, and distinguishes *all* the cells of the one race from those of the other. It is these properties with which geneticists deal. This enables us to understand how it might happen (as has been shown to happen in some cases) that cells from the "black" specimen from a part which would not ordinarily exhibit skin cells, might be transplanted to a "skinny" situation on the "yellow" embryo, and would there behave "ortsgemäss" but would still elaborate not "yellow" but "*black*" granules. Transplanted parts retain their "species specificity." The skinny situation furnishes the particular *organic relations* requisite for the development of *granules* (as contrasted with other possible histological elaborations), but since the particular color correlated with the granules depends upon the intrinsic properties, not on the relational properties (in the sense here used) of the cells, the particular granules in this case will still be "black."

So far so good. We have seen how it is that all the cells may have a complete stock of genes, and how all of the genes *may* be involved in the elaboration of what is necessary for the manifestation of a single character, *provided* we are willing to recognize the difference between intrinsic and relational properties of non-cellular parts, and the existence and importance of internal relations in an organic hierarchy. Such possibilities were closed to Weismann because he assumed that the properties of a cell (non-cellular part) depend *only* on its nucleus (at least during development) and thus completely ignored the mutual internal relations between parts. He was therefore driven to assume also that "there are material particles in the germ each of which is to be regarded as the primary *Anlage* of one portion of the organism," and that "the chromatin which controls the properties of cells must be different in each kind of cell." (Thus chromatin takes the place of Descartes' God as the "controlling" mechanic). In conformity with the age in which he lived Weismann also asserted that these assumptions *must* be accepted as correct *for all time to come!* (12). Dogmatism is a deep-seated vice of human nature not confined to theologians (i.e., not a specific relational property!).

But now comes the difficulty. We have all along been taking these specific organic relations in the hierarchy for granted, whereas these relations themselves develop or come into being and this itself constitutes one of the problems of embryology. Development does not consist simply in a process of cell-division followed by one of

histological elaboration. Moreover, we started with a black patch but we have been considering only its blackness and forgetting its shape, size and (perhaps) symmetrical relations to the whole. These are also characters. In considering the blackness of the patch we could concentrate on one process, namely the elaboration of intra-cellular pigment granules with certain properties (i.e., the property of having a *black* color correlated with them for an observer). This might have been the case even although only one cell had been involved. But our patch may involve thousands of cells, and consequently in dealing with its size, shape, and relation to the whole we cannot confine ourselves to what is happening in individual cells. All this means that there is another developmental process to be considered, namely the elaboration of *cellular* parts, of parts, that is to say, which are analysable into cells in certain determinate relations, and which thus constitute parts belonging to a level above that of the cells. It should be remembered that it is upon these parts and their relations (in so far as they differ in different districts in the whole) that we have been driven to place the "responsibility" for the differences in the histological elaborations of the various non-cellular parts which (in their relations) constitute them.

We must go back again to the beginning of development. Conceive an ovum which undergoes a perfectly equal holoblastic cleavage, resulting in a blastula with a perfectly uniform wall consisting of cells all having precisely the same original *intrinsic* properties. This would be an "harmonious equipotential system." Let it undergo gastrulation. Now the causal postulate requires that there should be some difference between the part of the gastrula wall which is invaginated and the rest—either in the organism or in the environment. But by hypothesis it seems that there is no difference in these two parts of the organism, and if we are to rely on the environment then gastrulation would depend (and which particular part is invaginated would depend) on a mere environmental contingency. And this does not seem to be the case. But, it will be said, it is useless to consider such a hypothetical case because no such thing ever happens—the ovum always exhibits some polarity which, in the blastula, is differentiated among the cells of the two poles, and it is this which is responsible for the differences in their behavior. Very well then, let there be some such difference, say of size, as seems to be the case in *Amphioxus*, and let this be a sufficient difference to account for gastrulation, as Assheton tried to show (1). This would be a case of differentiation by unequal cytoplasmic division with respect to volume. But this will only help us over one stile and there are very many to cross. Having got our gastrula we now have two cellular parts established: ectoderm and endoderm. We thus have some scope for different relations in the hierarchy. But is this sufficient? What is to "make the difference" (as common-sense folk always say) between right and left halves, dorsal and ventral surfaces? Moreover, we next require differences between the different parts of the archenteric roof to enable us to understand how it is that some become mesodermal pouches, and one becomes notochordal tissue. We can hardly go back to a cytoplasmic "preformation" for these latter differences, because it seems clear from various experiments that they involve relational properties and depend primarily on the blastoporic lip. If we are to assume that some minute part of the egg-cytoplasm is destined to form the organizator how will this enable

us to understand the *differences* in the parts of the region it organizes? And even this will not take us very far. As development proceeds more and more cellular parts are elaborated, and if these are all to be referred back to the egg-cytoplasm the latter will become, it seems, intolerably overcrowded. Are all the cellular parts of a man represented in the minute amount of cytoplasm of the human ovum? What, then, shall we say in those cases in which it is possible to chip off pieces of cytoplasm in any direction without preventing the elaboration of cellular parts? How shall we interpret "identical" twins? Does each blastomere possess a complete preformation? It seems clear that a cytoplasmic preformation is as difficult to believe in as Weismann's nuclear one.

Let us then review the possibilities regarding the modes of elaboration of cellular parts as they appear to our present ways of thinking. We seem driven to choose between: (1) a nuclear preformation; (2) a cytoplasmic preformation; (3) environmental differences during the ontogenetic period; (4) appealing to a transcendent "principle"; (5) rejecting the causal postulate. Of these (1) and (3) seem to me to be excluded by modern experimental data as decisively as anything can be excluded by experiment. I can see no possibility of help from (4). This leaves us with (2) and (5), which are not so simple and clear cut as might be supposed at first sight. Even Driesch shrinks from rejecting the causal postulate, at least in the form of the "principle of sufficient reason." And to suggest such a thing in scientific circles is like uttering one of the nine unprintable Anglo-Saxon monosyllables at a polite tea-party. But nevertheless there is still scope for plenty of discussion regarding the use of the causal postulate in embryology. To do this adequately here would carry us far beyond the confines of a single paper. It should, however, be understood that *if* there are equipotential systems, and *if* we are bound by the principle of sufficient reason, then it would absolutely follow that there were some such entities as Driesch's entelechies. I doubt whether the majority of embryologists have fully realized this. But to explain this fully would also take up too much space. All that seems to remain is some kind or degree of "cytoplasmic preformation." Would it be possible to appeal to just enough cytoplasmic preformation to give us our main axes and the primary germ-layers, and could we from thence forwards appeal on a large scale to relational properties? At first the latter would be involved in the blocking out of the main districts during the earlier periods. When these are "set" in accordance with relational properties they behave "*herkunftsgemäss*" on transplantation; hence their morphogenetic properties are now "acquired intrinsic" although *relationally* acquired, as shown by the totally different result of an earlier transplantation. But within each of these main "blocks" there will presumably be sub-parts which undergo elaboration in accordance with relational properties, until finally we reach histological elaboration as already explained. Moreover this "minimum" cytoplasmic preformation to which we are appealing will itself be the outcome of intra-cellular elaboration in accordance with the ovarian relations furnished by the maternal parent. And this enables us to burden the chromosomes with no more than the duty of providing what is needful (whatever that may be) for the various possible histological elaborations of which a given race is capable. This is all that "genes" need to do apart, of course, from persisting and dividing.

This possibility, then, has its attractions. But will it square with "identical" twinning and with cutting and transplanting experiments? Does it put too much upon relational properties? These are questions requiring further thought and experiment. Experiments should be devised for testing relational properties in greater detail and with greater precision. Also it is very important to know whether there *really are* equipotential systems in the organic realm. A great deal hangs upon this. To sum up: the crucial problems of embryology seem to be those concerning the three principal modes of elaboration—of cellular, non-cellular and of cell-parts. And it seems perfectly clear that it is the *primary* duty of the embryologist to discover how to interpret the developmental process in terms of the intrinsic properties of the various "genetic types" of cells and the relational properties they exhibit in the several hierarchic organic relations which arise in the course of the elaboration of non-cellular parts, and which are accessible to our observation. And this primary objective is quite independent of any theoretical views we may hold regarding the ultimate "reducibility" of biological to other branches of natural knowledge. It is not the business of the biologist to lose his way in a fog of hypothetical imperceptibles. I agree with the physicist who wrote: "I have no doubt whatever that our ultimate aim must be to describe the sensible in terms of the sensible." If Lavoisier, at the foundation of chemistry, had proceeded on the assumption that it was the business of chemistry to reduce itself to physics there would probably never have been any chemistry. The moral for biology is obvious. There is no short cut to biology if it cuts blindly across the observable facts of biological organization. Not to recognize this means tying biology down either to the "verification" of the metaphysics of Herbert Spencer, or, if it is only interpreted methodologically, to an extremely restricted circle of thought devised, in the first instance, on account of its value in a different sphere. This does not apply, of course, to genuine bio-physics and bio-chemistry in their own fields, but to vague speculations in other branches of biological inquiry which merely borrow their terminology and ape their manners.

The "concept of organism" seems to be forcing itself upon our attention from three directions: First, from that of embryology and physiology. The notion of a morphogenetic "field" in Gurwitsch (6) and Weiss (13) is highly symptomatic of this movement and deserves attention. Secondly, there is the *Gestalt-theorie* beginning in psychology and extending to physics (8). Thirdly, there are those philosophers of evolution—Hobhouse, Alexander, Lloyd Morgan, Smuts and Whitehead—all of whom employ the concept of organism in various ways and degrees not exclusively biological. Perhaps this concept will enable us to understand evolution. Has anyone observed a machine that was capable of evolution *without a mechanic*?

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HISTORICAL SURVEY OF THE DEVELOPMENT OF THE SYMPATHETIC NERVOUS SYSTEM

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INTRODUCTION

THE development of the sympathetic nervous system has been and still is one of the most discussed problems of vertebrate embryology. During the last sixty years the literature on this subject, representing the investigations of a very large number of workers, has become extremely extensive and it reveals no agreement concerning the most fundamental points of the question. The researches were carried on in all groups of vertebrates; the anatomical findings were corroborated by embryological observations; the experimental procedure was applied in various ways; still our knowledge concerning the origin and the development of the sympathetic nervous system is very incomplete and scattered.

It was our purpose to carry on new investigations in this field, mostly from the embryological standpoint, supporting the morphological observations by experimental demonstration.

The literature is always partly reviewed by the many investigators, but no complete survey is to be found. The anatomy, the physiology and the pathology of this system are extensively discussed in many publications, among which the outstanding are those by Langley, Langdon Brown, Guillaume and L. R. Müller. The embryological aspect is generally very briefly summarized or overlooked; it may even seem extraordinary to have physiological or pathological discussions, classifications and theories without any serious knowledge of what the sympathetic nervous system is, where it comes from and how it develops. We believe that before presenting our original contribution to the problem, it might be useful to offer a complete review of the embryological facts hitherto described and to try to get out of it, impartially, a few positive problems to be studied in order to establish a definite theory about the development of the sympathetic nervous system. In making this review, we shall follow the chronological seriation, grouping however, the different publications of the most important investigators; we shall also group the most important data concerning the development of the chromaffin system.

The term "sympathetic nervous system" is used with many different meanings, giving rise to many misunderstandings; actually it has been replaced by Langley's denomination "autonomic nervous system" (involuntary nervous system of Gaskell, système nerveux organovégétatif of Guillaume, Lebensnervensystem of L. R. Müller). The autonomic nervous system includes the ortho- and the parasympathetic systems, as well as the intravisceral nervous structures.

Our review deals in fact with the autonomic nervous system of the trunk;

it represents all the embryological data concerning the thoracolumbar sympathetic chains and the visceral part of the vagus complex. A few references are given to the development of the head sympathetic nervous system. These will represent the actual state of that aspect of the question, and in them the complete bibliography will be found. We shall not consider the purely anatomical facts nor refer to the physiological experiments unless they offer a striking confirmation or support to the embryological observations; neither shall we report any phylogenetic theory elaborated in comparing the autonomic nervous system and the nervous organization found in invertebrates.

This survey is to be the introduction to a series of studies concerning the development of the autonomic nervous system; as such, and in order to avoid frequent repetitions, we shall refer to it as a bibliography. The recent book of A. Kuntz on the autonomic nervous system appeared too late for critical review in this paper.

REVIEW OF THE LITERATURE

Histogenesis of the Autonomic Nervous System

Remak in 1847 was the first scientist interested in the anatomy and the embryology of the sympathetic system. He suggested that the primordia of the sympathetic ganglia were formed by proliferation of mesodermic elements at the point where the communicating rami arise from the spinal nerves.

The first appearance of these anlagen is seen in the chick embryo at the end of the fourth day of incubation; during the sixth day a big nerve trunk, called since then the nerve of Remak, develops all along the intestine. This nerve, also probably mesodermal in origin, was described as entirely independent of the sympathetic cords. Nearly thirty years later Goette confirmed that conception without bringing in any new observations.

The first important embryological contribution to this subject was made by Balfour ('78) in a series of papers dealing with the anatomy and the development of the elasmobranch fishes. He showed the first trace of the sympathetic system arising as short branches from the spinal nerves, directed toward the median line of the body and terminating in small irregular cellular masses lying dorsal to the cardinal veins; these cellular clusters always retain their connection with their respective mixed spinal nerve by a small fibrous branch which will become the communicating ramus. In the young stages there are no apparent intersegmental commissures, the sympathetic cords being secondary structures; the most anterior sympathetic ganglion is in the heart region close to the end of the intestinal branch of the vagus; the last one being at the very end of the abdominal cavity. From the cellular prevertebral heaps, fibrocellular branches may be followed to the intestinal tract; he suggests the schematic formula that each spinal nerve divides into three branches: a dorsal, a ventral and an intestinal one. One year later Schenk and Birdsall applied the ideas of Balfour to the upper classes of vertebrates. In a chick embryo of five days incubation, as well as in a human embryo measuring twenty-two millimeters, the sympathetic primordia are not yet developed; but from the distal end of the spinal ganglia arise groups of cells migrating along the dorsal roots and in the mixed spinal nerves. These migrating elements, keeping their connection with the spinal nerve, become the anlagen of the sympathetic system. From these prevertebral ganglia arise new branches contributing to the formation of the prevertebral

plexuses and of the visceral plexuses; the Auerbach plexus is directly connected with the sympathetic cords.

In 1886, Onodi published a very extensive piece of work dealing with the development of the sympathetic system in the entire vertebrate series, showing clearly that the sympathetic trunks and the prevertebral plexuses arise from the central nervous system, more exactly from the spinal ganglia. The migration of cells from the distal end of these ganglia is due to the pressure exerted by the newly formed cells within the ganglion; it is thus a purely passive migration. He was unable to detect the origin of the peripheral sympathetic plexuses and suggests their mesodermal origin for lack of other evidence. In *Scyllium* the first anlagen of the sympathetic ganglia are seen as triangular thickenings of the ventral pole of the spinal ganglion in every trunk segment; the cells forming those thickenings result from mitotic processes in the spinal ganglion cells, and show in themselves a very marked karyokinetic activity, the result of which is their migration toward the dorsal and lateral aspects of the aorta and along the small vessels running in the root of the mesentery. The sympathetic ganglia so formed are then connected with the spinal ganglia by small communicating rami; later on they send commissural branches to each other, thus forming the sympathetic chains. Van Wijhe ('89) briefly refers to the sympathetic system in describing an embryo of *Pristiurus* at the stage of 84 somites; he shows that each segment of the suprarenal organ with its corresponding sympathetic ganglion is derived from a cellular proliferation in the spinal nerves, the proliferating and migrating cells remaining always connected with the spinal nerves by fine nerve fibers.

Paterson ('90) believed that the sympathetic structures arise as a continuous, non-segmented cellular cord, the cells of which are derived from the mesodermal tissue; the segmentation of this chain is only secondary to the establishment of late connections with the series of spinal nerves and to the relations with the vertebral column. W. His ('79) in a 7 mm. long human embryo did not find any sympathetic anlagen, while the spinal ganglia were well developed and the mixed nerves formed; immediately after this stage he sees short visceral branches arising from the spinal nerves a short distance beyond the junction of the anterior and posterior roots and growing toward the aorta. These branches are purely fibrillar at this time; cells showing clearly their nervous nature migrate later apparently from the spinal ganglia; from the retroaortic sympathetic ganglia nerve trunks grow out toward the visceral organs and constitute the peripheral sympathetic plexuses.

His, Jr., and Romberg ('90) showed that the sympathetic cells were not pushed out of the spinal ganglia or later from the prevertebral sympathetic ganglia as a result of increasing pressure behind them through a rapid cellular migration, but that they were actively migrating into the surrounding tissue; the cells are at this stage apolar, and there is no indication at all of any deformation that would be the result of a passive pushing out by pressure. From the ganglia of the sympathetic cord the cellular elements wander out ventrally to give rise to the coeliac and other peripheral ganglionic masses. This peripheral migration is clearly described for the ganglia of the cardiac plexus in fishes, amphibians, birds and man; it appears to occur along the veins in fishes and amphibians, along the arteries in birds and mammals.

Golowine ('90) studied the development of the ganglionic system in chick

embryos; his work deals mainly with the relations existing between the nervous system and the sense organs. He considers the neural crest as originating from the ectoderm in common with the sense organs, independently of the nervous system. The sympathetic system originates from the ventral part of the spinal ganglia, when these are not yet fully differentiated; the anterior roots are at first cellular and appear only when the sympathetic ganglia are becoming independent of the spinal ganglia.

Goldberg ('91) verified the descriptions of His, Jr., without bringing in any new facts.

In a four day chick embryo, His, Jr., ('92–'93) showed that the clusters of apolar sympathetic cells found in the mesenchyme between the aorta and the point of junction of the spinal roots are evidently derived by migration from the spinal ganglia. In a later paper published in 1897 he described the succession of two sympathetic chains; the first one appears at the end of the third day of incubation and forms a continuous ganglionic cord on each side of the aorta, an abdominal plexus and a pelvic cord. That chain becomes less and less distinct and has entirely disappeared during the eighth day; at this moment the ventral pole of the spinal ganglia shows the wandering out of a new generation of migrating elements contributing to the formation of the secondary and permanent prevertebral chains. This description of His, Jr., is the first complete study of the development of the sympathetic system, based on a series of embryonic stages; it represents the most careful piece of work in this field hitherto published and its conclusions appear perfectly justified.

In 1898 Fusari studied the development of the sympathetic nervous system and of the adrenals in chick and mammals. In the chick embryos he describes the first anlagen of the sympathetic trunks around the stage of sixty hours; at that stage the spinal ganglia are developing, but their peripheral branches do not yet reach the ventral roots and are far away from the sympathetic anlagen. A continuous longitudinal sympathetic chain is constituted on either side of the aorta before any connection can be traced between it and the spinal roots either ventral or dorsal; the communicating rami appear only later by the growth of fibers from the sympathetic ganglionic masses. The intestinal nerve of Remak seems to be at first independent of the sympathetic anlagen, with which it later acquires connections. The adrenals are formed by the intermingling of true mesodermal elements with others derived from the sympathetic anlagen. The sympathetic system seems to be derived from the elements of the protovertebrae and is not genetically related to any part of the central nervous system.

In a short paper published in 1894, G. Mazzarelli dealt with the origin of the sympathetic in various vertebrates, including selachians, amphibians, reptiles and birds. In *Passer* the youngest stage, for which no exact age or measurement is given, shows bands of isolated cells migrating from the ganglion crest region toward the lateral aspect of the aorta; these cells are doubtless neuroblasts. In a later stage this migrating band divides transversally into a dorsal part giving rise to the spinal ganglion and a ventral one representing the anlage of a sympathetic ganglion. This second stage described by Mazzarelli seems to correspond to the youngest stage studied by Paterson and Fusari, who in that way misunderstood the primary origin of the sympathetic ganglia. Mazzarelli's conclusions are also concerned with other groups of verte-

brates, though the migration is much less important in birds than in selachians and is still less marked in mammals.

In a series of papers, Dogiel ('95–'96) described the microscopic anatomy of the sympathetic ganglion, as well in the prevertebral chains as in the visceral organs. A very important result of his observations is the demonstration of two types of nerve cells in the intestinal plexuses. The first type is identical with the motor elements of the cerebrospinal nervous system, with short, very richly ramified dendrites and one long neurite passing more or less deeply into the musculature. The second type represents cells with from two to ten dendrites coming from the glandular and epithelial structures and one neurite going toward the type I cells; the dendrites are supposed to be receptors of stimuli which are conveyed by the neurite to the motor cells. Both cellular types are to be considered as sympathetic in origin, although Dogiel did not try to base his assumption on embryological studies.

Kölliker ('94) verified the origin of the sympathetic system as a result of the migration of cells out of the spinal ganglion and identified all the peripheral plexuses as sympathetic derivatives. The same year, Rabl described the first anlagen of the sympathetic system in *Pristiurus* at the stage of seventy-four somites as a thickening along the spinal nerve, due to an elongation of the corresponding spinal ganglion.

In 1900 Hoffman studying the development of the sympathetic system in selachians found its first appearance in a 15 mm. embryo of *Acanthias vulgaris* as cellular heaps to which the contribution of the spinal ganglion seems much greater than that of the motor roots. These cellular masses increase in size and differentiate into a medial part, which becomes the sympathetic ganglion, and into a lateral one mostly fibrillar, which gives rise to the ramus communicans. The sympathetic chains show segmental ganglia except for the first three cervical segments, the first trunk ganglion being connected with the fourth cervical nerve; the sympathetic ganglia corresponding to the four or five following spinal ganglia fuse to give rise to the superior cervical ganglion. Taking into account the fact that the sympathetic trunks appear only after the constitution of the mixed spinal nerves and arise near the junction of both roots, Hoffman concluded the necessity of that junction for the development of the sympathetic anlagen. In the head, where such junctions do not occur, no sympathetic ganglia are to be found except at the connection between the ophthalmic nerve and the oculomotor nerve, the ciliary ganglion being the result of this junction. In the idea of Hoffman, the ciliary ganglion is the only true sympathetic ganglion in the head. In 1902, studying the development of the sympathetic system in urodeles, he described the first sympathetic anlagen as being connected by narrow strands to the ventral roots as if the cellular migration of the sympathetic elements had been accomplished along the ventral roots.

O. Schultze ('94) in embryos of *Vespertilio* found the first trace of sympathetic elements as an unsegmented chain running in the corner between the aorta and the intersegmental arteries. The sympathetic elements there located seem to arise from a differentiation of mesoblastic elements, or more exactly, he found no reason to admit that they migrate from the spinal ganglia.

Onuf and Collins ('00–'02) published a very extensive piece of work about the central localization of the sympathetic nervous system with a critical review of its anatomy and physiology. This paper, frequently overlooked by later authors, is a

very important contribution to the problem of the sympathetic system and is one of the first experimental studies with an anatomical purpose. Before discussing these researches we want to say a few words about a very ingenious theory advanced by Jendrassik ('99). This investigator divides the sympathetic nervous system into the spinal and the vagus systems. The spinal system includes the ganglionated cords and the peripheral ganglia located near or in the different organs; the vagus system is represented by the vagus nerve. The vagus nerve should be considered as a purely sensory nerve, the motor fibers running with it belonging in fact to the neighboring nerves; the branches of the vagus nerve to the organs convey the sensory impulse to the jugular ganglion and to the medullae-oblongatae nucleus, from which fibers exclusively motor in function go to the spinal system.

This theory, however interesting it may be, is strongly contradicted by very positive observations of Langley ('03) and Kölliker ('94). These two authors proved that the sympathetic system contains at the same time centripetal and centrifugal fibers, and that in the centrifugal fibers some were motor and others inhibitory; the purely sensory function of the vagus is, therefore, to be discarded! However, nothing definite is yet known concerning the possible origin of the sensory sympathetic fibers nor their connection with the cerebrospinal system. Kölliker claims that all the sensory fibers of the sympathetic system originate from cells of the spinal ganglia in exactly the same way as do the sensory cerebrospinal fibers. Dogiel ('95) is inclined to assume the existence of specific sympathetic sensory fibers arising from the cells of the sympathetic ganglia or plexuses. Sala ('93) describes two types of fibers in a sympathetic ganglion; the varicose fibers and the dividing fibers. The varicose fibers, presenting a tortuous course, remain in the ganglion itself without giving off any collateral branches and are to be considered as true sympathetic fibers originating from cells of the sympathetic ganglia; the dividing fibers send off numerous collaterals, the ramification of which forms the diffuse network of the ganglion. These only are derived from the cerebrospinal system. Gaskell ('86) noticed for the first time that while the communicating rami were composed of medullated fibers, the efferent rami of the sympathetic ganglions contained non-medullated ones; he was also the first to carry on a combined anatomophysiological experimentation on the visceral innervation of the crocodile. In the young crocodile the rami communicantes between the spinal nerves and the ganglionic chain are very short and the spinal ganglia are very near the corresponding sympathetic ganglia; in the tortoise the ramus communicans in the thoracic region springs directly out of the posterior root ganglion, while according to Giacomini in *Bufo* and *Bombinator* the two ganglia actually touch one another.

Gaskell cut the vagus nerve above the ganglion trunci vagi and stimulated the nerve at the peripheral cut end, obtaining a strong peristaltic contraction of the whole oesophagus and of the stomach; the same effect takes place when the stimulus is applied below the ganglion. After allowing degeneration, the stimulation above the ganglion does not produce the slightest effect on the oesophagus or the stomach; the same stimulation below the ganglion causes a marked contraction except in the cervical part of the oesophagus. These experiments prove that the fibers for the thoracic portion of the oesophagus

and for the stomach are interrupted by the cells of the nodosum ganglion while those for the cervical oesophagus are not.

In removing a few sympathetic ganglia without touching the spinal ganglia Onuf and Collins ('00) observed a degeneration of the cells of Clark's column and a degeneration of fiber bundles going from the posterior root to Clark's column; the fiber degeneration exists at the level of the sympathetic removal, while the cellular degeneration is only to be found a few segments higher. Thus the sensory fibers of the sympathetic system do not originate from spinal ganglion cells but from those in the sympathetic ganglia; the sensory fibers after having reached Clark's column make an ascending course to and around Clark's cells at a considerably higher level. The extirpation of both stellate ganglia produces atrophic changes in the lateral horns, both paracentral groups and both columns of Clark from that level down to the ninth dorsal segment; the downward extension may be due either to a descending extension of fibers in the spinal cord or in the sympathetic chain.

In 1905 Jones published a short paper on the development of the sympathetic nervous system in the toad. Although brief and rather incompletely worked out, it has the great interest of bringing forward important observations. Between the vagus ganglion and the second spinal nerve he describes at the 9 mm. stage sympathetic cells scattered in the mesoblast dorsolaterally to the aorta; these cells show a tendency to aggregate themselves in an irregular longitudinal cord, which becomes well defined at 12 mm., being followed continuously down to the sixth spinal nerve. At the level of the fourth spinal nerve exists an enlargement corresponding to the position of the adrenals. At 14 mm. in length the rami communicantes can be seen as fibrous branches connecting the primarily isolated sympathetic clusters with the mixed spinal nerves; at 21 mm. there exists a very intimate connection between the vagus ganglion and the anterior end of the sympathetic chain.

Neumayer ('06) in *Lacerta* finds the first sympathetic anlagen in an embryo in which the auditory vesicle is not yet entirely separated from the ectoderm. On the medial side of the mixed spinal nerve, already fibrillar but still presenting numerous migrating cells, he describes a small fibrocellular branch directed medially and ventrally toward the aorta. Somewhat later the branch itself, purely fibrillar, becomes the communicating ramus, while its end is thickened in a cellular cluster, the primordium of the sympathetic ganglion. The sympathetic anlagen are to be considered as cellular buddings of the spinal nerves.

In an 11 mm. embryo of *Pristiurus*, the ganglia appear as small buds of the mixed spinal nerves, near the junction of the ventral and the dorsal roots; from those cellular heaps, small branches are growing along the mesial aspect of the mesonephros. Later on, the primordium of the sympathetic ganglion divides into a mesial portion, cellular, and a lateral one representing the communicating ramus; from the ganglion, a few fibrocellular branches extend in the mesentery and spread into the walls of the intestine. Fusari also studied the development of the sympathetic system in mammals and concluded that it is derived neither from the spinal ganglia nor from the neural tube, but is the result of a local differentiation of the elements of both roots, exactly in the same way as the spinal ganglion cells and the nerve fibers are derived from elements differentiating in situ.

In 1905 and 1907, Kohn studied the histogenesis of the sympathetic nervous system in mammals, especially in the rabbit;

this description might be taken as a model of embryological study of the sympathetic system. In eleven-day old embryos he does not find any trace of the sympathetic system. Six hours later he observes the first step in the migration of sympathetic elements. Their origin is altogether from the neural tube along the anterior roots and from the spinal ganglia. The emission of neurocytes from these two sources does not continue a long time, but numerous mitoses occur in them, resulting in the progressive multiplication of sympathetic elements. In eleven days and fifteen hours, the migrating neurocytes reach the lateral aspects of the aorta, and at this level they accumulate, forming small ganglia, very rapidly acquiring connections with each other and constituting an uninterrupted reticulum along the body of the embryo. In twelve days, the sympathetic system begins to become independent of the central nervous system as far as cellular contribution is concerned, the rami communicantes becoming purely fibrillar. On the thirteenth day the network aspect of the sympathetic system disappears in the anterior region of the embryo, where a true cellular cord is formed without segmental thickenings. The amount of sympathetic tissue increases more and more, especially at some levels such as the adrenals and the gonads. Kohn doubts, however, whether all those sympathetic elements are directly derived from the prevertebral chains, because for the gonads at least the sympathetic chain has probably not yet grown so far back. On the fourteenth and fifteenth days three regions can be distinguished in the sympathetic cord. In the cervical region it is purely a cellular, well defined band extending to the level of the pharynx. The medial or trunk region shows a fibrocellular chain, while in the caudal region, caudally to the pronephros, it keeps the reticular aspect of the primary anlagen. In sixteen day embryos appear the sympathetic ganglia with the fibrillar connecting commissures. The differentiation of the sympathetic cells in the chain begins only at the seventeenth day to be complete around the twenty-first, while the elements of the peripheral ganglia are already differentiated in typical ganglion cells around the seventeenth day.

Poll in 1904, studying a complete series of *Emys europaea*, described very completely the origin of the cortical part of the adrenal from the peritoneal epithelium and of the chromaffin part from the anlagen of the prevertebral plexuses. In a later paper he dealt with the organogenesis and the histogenesis of the chromaffin tissue and traced the evolution of the primitive sympathogony into either sympathoblast and later sympathetic ganglion cell or phaeochromoblast and phaeochromocyte.

Whitehead ('03) in a 13 mm. long pig embryo described sympathetic ganglia located dorsolaterally to the aorta, nearly opposite the dorsal border of the Wolffian body and connected with the spinal nerves by splanchnopleural branches; numerous smaller collections of sympathetic ganglion cells with many mitotic figures could be seen scattered on each side of the aorta. At 16 mm. chains of ganglia of various sizes appear between the aorta and the cortical anlagen of the adrenals; from these ganglia nerve fibers grow out and accompanied by numerous cells run lateralwards along the ventral and the dorsal aspects of the adrenals. At 30 mm., the capsule of the adrenal primordium breaks up, and a direct connection is achieved between the sympathetic ganglion cells and the cortical part of the adrenal. The cortex develops a long time before the migration of sympathetic elements; it is first seen in 8 mm. embryos and is derived from the peritoneal epithelium.

In 1907 Froriep, studying embryos of *Torpedo* and of the rabbit, described medullary cells migrating along the ventral roots of the spinal nerves. In a first paper he considered those cells as giving rise to the neurilemma, but after the conclusive experimental results of Harrison ('04) showing that the sheath cells of both ventral and dorsal roots take their origin in the neural crest cells, he concluded that the migrating cells found along the motor roots and from the spinal ganglia along the dorsal roots give rise to the sympathetic nervous system. He asserted that all the sympathetic neurones in the prevertebral chains and plexuses arise from the ventral half of the nervous system and he looked toward the realization of experiments proving that the removal of the neural crest does not interfere with a normal development of a complete sympathetic system.

London and Pesker ('06) studied the development of the peripheral nervous system in mammalian embryos. Their researches deal with the histogenesis of the nerve endings in striated and in smooth muscles, in heart muscle and in cutaneous epithelium. Their paper represents the first attempt to study the embryology of the nervous endings, but it is too incomplete to draw definite conclusions. The nerve endings in the smooth muscle develop in the same way as those in the striated musculature, although there is some shortening of the developmental stages. The heart ganglion cells are considered as true sympathetic cells, as had already been shown by Aronson, His, Jr., Arnstein and Smirnow.

In a series of papers Streeter ('08, '12) described the peripheral nervous system of human embryos; though no very extensive account of the sympathetic nervous system is to be found, many interesting details are described dealing with the neural crest in the occipital region and the distribution of the cranial nerves. In a 10 mm. embryo the peripheral migration of sympathetic elements is completed, forming on each side of the body a longitudinal column, lateral to the aorta, extending from the occipital region to the lower sacral vertebrae; from the sixth to the tenth thoracic segments a definite fibrocellular plexus forming the splanchnic nerves and the coeliac plexus is found.

Carpenter and Main ('07) studied the migration of medullary cells into the ventral nerve roots of pig embryos. In embryos 11 mm. in length, breaches of considerable extent appear in the external limiting membrane of the spinal cord; through these, medullary cells are passing into the nerve roots. Such cells can be seen just inside the external limiting membrane, in an intermediate position half in and half out of the neural tube and in the basal part of the nerve root just outside the tube. Harrison saw also medullary cells migrating into the ventral nerve roots of salmon embryos and had already suggested the possibility that these cells may follow the visceral branches of the spinal nerves into sympathetic ganglia and there give rise to motor neurones. Carpenter and Main emphasize the difficulty of a definite answer to the question whether or not any of those migrating cells corresponding to the indifferent cells of Shaper, become the nerve cells of the sympathetic ganglia; they note with interest that Kölliker described true ganglion cells in the ventral nerve roots of adult cats.

Cajal ('90, '92, '95) reserved his opinion as to whether the sympathetic neuroblasts migrate along the ventral or the dorsal roots of the spinal nerves. Using the silver impregnation method on chick embryos, he tended to the view that they wander out along the ventral roots before

the moment the anterior roots become fibrillar and when the cells are still apolar.

Held ('09) published a very important piece of work dealing with the peripheral nervous system in the entire series of vertebrates. He tried to show that in all vertebrates the cells found along the motor roots do not play any rôle in the development of the sympathetic system, which is entirely derived from the spinal ganglia. In a 12 mm. embryo of *Acanthias*, the dorsal root is extraordinarily rich in migrating elements, while the motor root is practically fibrillar; the fusion of both roots to form the mixed spinal nerve is found more ventrally than it usually is, at a point where the dorsal root has already given off its sympathetic thickening. In all the selachians, the sympathetic cellular aggregates appear at the median side of the spinal nerves. Inasmuch as the fibers of the sensory and of the motor roots of the spinal nerve do not mingle with each other but grow peripherally as distinct fiber bundles, of which the sensory lies mesial to the motor, the cellular elements of the sympathetic anlagen are in immediate contact with the sensory fibers and have no apparent relation to the motor fibers. Anatomically and embryologically the sympathetic anlagen are exclusively connected with the dorsal roots and with the spinal ganglia. In amphibians the earliest trace of cellular migration from the spinal ganglia is observed at 5.75 mm. In this group, cells migrating along the ventral roots never occur, and here at least the sympathetic anlagen have necessarily their origin in the spinal ganglia. Furthermore, the physiological experiments of Steinach ('94) on *Rana esculenta* have proved that the posterior spinal roots contain visceral motor fibers to the stomach and the intestinal tract. In the other groups, Held admits the real existence of a cellular migration along the ventral roots. The migration, very abundant in the reptiles, is very poor in other vertebrates, but he does not eliminate definitely a possible rôle of those cells in the formation of the sympathetic anlagen and he leaves the question unanswered.

Marcus ('09) stated that the cells described by Froriep along the ventral roots do not migrate from the spinal cord but from the neural crest. In early stages of *Torpedo* he was able to observe cellular chains connecting the neural crest with cell aggregates situated in the ventral roots, those connections being found before the first differentiation of the spinal ganglia. He, therefore, admits that the neural crest represents the only primary source of the sympathetic neurones.

Hoven ('10) studied extensively the histogenesis of the peripheral nervous system in chick embryos. Though the most important part of his work deals with the cytological processes of differentiation of the neurofibrils, he gives interesting data concerning the organogenesis of the nervous system. Using the silver nitrate impregnation methods, he shows that the very first appearance of the ventral roots as growing neurites of intramedullary neuroblasts takes place at 76 hours of incubation. He never found any migration of cells along the motor root and considers that the cells described by Balfour ('78), Marshall ('78), Dohrn ('91), Pighini('04), Froriep ('07), Held ('09), Carpenter and Main are purely mesenchymal cells. The sympathetic ganglia are derived from the spinal ganglia and are very clearly observed at 79 hours of incubation. For him the sheath cells are of mesodermal origin.

In numerous papers Ganfini ('08, '12, '14, '16) described the sympathetic trunks arising from neurocytes which advance peripherally along both the dorsal and ventral roots of the spinal nerves. The

enteric plexuses are developed from the anlagen of the sympathetic trunks, from which cells migrate further ventrally than those contributing to the formation of the prevertebral plexuses. We shall refer to this investigation later on. In a short paper, Meikeljohn ('08) studied the development of the visceral nervous system of the digestive tract in the chick, by the silver impregnation method. At five days, vagus fibers are described entering the stomach, forming in its wall a primordium of Auerbach's plexus. The plexus is formed by fine fibers given off by mono- or bipolar cells. In the lowest part of the intestine, a few bipolar cells and a few fibers seem to rise from the cord in the position of the nervi erigentes. At six days the vagal contribution to the stomach is more abundant and there has developed a rich network of delicate fibers, apparently representing the sympathetic supply. In the lower part of the intestine, a chain-like arrangement of cells and numerous fibers is found innervating its cloacal end. The other parts of the intestine show a few scattered cells and fibers without clear plexiform arrangement. This preliminary note, illustrated by very schematic drawings, shows the important part played by the sympathetic system in the innervation of the alimentary canal; the oesophageal plexus and the gastric Auerbach's plexus are the only structures derived from the vagus and are soon invaded by the sympathetic supply.

W. Lewis and M. Lewis ('12) studied a culture of sympathetic nerves from the intestine of chick embryos in saline solutions; the growth of the nerve fibers occurs in ten per cent of the cultures and is observed with equal frequency in taking intestinal fragments of chick embryos from eight to thirteen days of incubation.

In the *Human Embryology* of Keibel and Mall, Streeter ('12) made a very important contribution concerning the development of the sympathetic nervous system in man. At 4.5 mm. of length cells from the loose ventral border of the spinal ganglion detach themselves and extend ventralward in advance of the ventral roots. The migration of sympathetic cells is accomplished by a pure wandering through mesoderm rather than along preestablished nerve fiber paths. By the time a well defined nerve trunk is differentiated, the sympathetic cells have already completed that part of their migration and the cells still found along the nerve trunks are only sheath cells. In a 7 mm. embryo the migration of cells into the sympathetic anlagen is in active process and cellular rami communicantes are present in some regions. At 9 mm. in length the ganglionic prevertebral cords and the splanchnic nerve plexuses are definitely outlined. At 6 mm. the visceral ganglia and the sympathetic ganglia situated in the head region are to be seen with their connecting branches. Carpenter ('06) had already shown that in the chick the ciliary ganglion consists of two portions, a small dorsal sympathetic part derived from the semilunar ganglion and a larger ventral part containing large bipolar cells derived from the neural tube by migration along the oculomotor nerve. Froriep ('07) described, in *Torpedo*, cells wandering out from the central nervous system together with the growing oculomotor fibers and fusing with the cells derived from the trigeminal ganglion so as to form a composite ganglionic mass. Streeter does not see any migrating cells along the oculomotor nerve in the human embryo. The ciliary ganglion consequently consists here exclusively of wandering cells from the semilunar ganglion. He assumes that as well as for the sympathetic anlagen of the trunk, their migration occurs very early, probably just in advance of the developing nerve fibers

and that it is not until they have reached their permanent position that they undergo an active proliferation and form a compact cellular mass. The sphenopalatine and submaxillary ganglia are probably derived entirely from the semilunar ganglion, though through their connections with the geniculate ganglion of the facial there is a possibility of a facial contribution. In the same way the otic ganglion is developed in intimate relation with the semilunar ganglion containing perhaps a few cells coming from the glossopharyngeal nerve through its tympanic branch.

R. Camus ('13) studied in considerable detail the development of the sympathetic nervous system in frog embryos. The first trace of sympathetic anlagen appears as isolated cells slightly different in cytological characters from the mesenchymal cells, presenting prolongations in direct continuity with those of the latter cells. In the spinal nerves no cellular elements are found besides the Schwann cells. For these two reasons he believes in the mesodermic origin of the sympathetic anlagen. At a later stage, the sympathetic ganglia are formed by a syncytium, the peripheral part of which is still in direct continuity with the mesodermal strands. About the same time appear the communicating rami, which the author identifies without any hesitation with the fibers of the mesenchymal net. At this moment the cells of the communicating rami, the true mesenchymal cells and the future ganglion cells are all in direct protoplasmic continuity. He admits further that the sheath cells arise from mesenchymal cells, the plasma of which differentiates in neural fibers. He describes neuroblasts in the intestinal wall at 6 mm. in length, giving rise to numerous independent small ganglia which become connected with the spinal nerves at the stage of 14 mm. Although this piece of work is a very voluminous one, one can not help being very sceptical about most of the observations. His isolated nerve cells either in the prevertebral region or in the intestinal wall are most probably ordinary connective tissue cells. The syncytial origin of the nerve fibers, the mesodermal origin of the sheath cells, the local differentiation of the communicating rami are only easy answers to questions unsolved by incomplete observations.

Miss Abel ('09–'12) published two successive papers on the development of the sympathetic nervous system in chick embryos; only the second one, a correction of the first one in the light of new observations, is to be taken into account. In a 60 hour embryo the silver impregnation shows in the mesoderm, close to the spinal cord, a few cellular elements larger and darker than mesodermal cells. At 72 hours occurs a very abundant migration of cells from the spinal cord into the spinal ganglia and from the ventral margin of the spinal ganglion into the beginning posterior root. At 80 hours of incubation there is a considerable increase of the cellular migration from the spinal ganglia forming a delicate chain running in the neighborhood of the aorta. A poor migration is also found along the anterior roots. These migrating cells are either indifferent cells of Schaper or true neuroblasts. At 108 hours the sympathetic chain is well developed on either side of the notochord behind the aorta, within the abdominal region a well marked ventral migration to the ventral aspect of the aorta. Somewhat later appear fibrillar communicating rami. In embryos of 144 hours the formation of the ganglia of the secondary or permanent sympathetic chain is first seen in the thoracic region. They are largely formed by the grouping of sympathetic cells which accumulate between the spinal nerves and the temporary sympathetic chain. The portion of the

latter which is not incorporated in the permanent chain gradually disappears. In the abdominal region the ventral migration of sympathetic cells to the abdominal and visceral plexuses was already so extensive that it is impossible to distinguish the temporary and permanent sympathetic chains. A special sympathetic supply is found along the vagus nerve, coming from the hind brain to the ganglia of the glossopharyngeal and vagus nerves. The cells found in this part of the sympathetic are characterized by the smaller cellular size and a less pronounced reaction to silver nitrate. From the vagal sympathetic supply, cells are found in the heart at 108 hours, in the stomach at the same moment, in the oesophagus, liver and pancreas at 144 hours. From the sympathetic prevertebral chains cellular outgrowths are penetrating into the walls of the gut around the stage of 144 hours, although in the large intestine the ganglion of Remak is already present at 108 hours and runs along that part of the digestive tract for a certain distance.

Roud ('03) believed in the mesodermal origin of the sympathetic nervous system, and Bruni ('16) extended in this sense his observations on *Gongylus*, chick and rat embryos. He described two successive anlagen. The first in a chronological order is mesenchymal, the second having an ectodermal origin. The mesodermic primordium is for the first time to be found dorsolaterally to the aorta at a position corresponding to the cephalic part of the mesonephros, and from this point it extends in both cranial and caudal directions. When this mesodermal sympathetic chain is formed, true neurocytes originating from the neural tube migrate towards the aorta and by the way of communicating rami the ectodermal generation of sympathetic cells becomes connected with the mesenchymal anlage. Following Bruni the mesenchyme-derived cells would act as chemotactic mechanism attracting the ectodermal cells, and he finds a basis for this idea in the existence of cytoplasmic granulations in the cells of the mesenchymal anlage. This anlage loses progressively its importance, its place being taken by the ectodermal neurocytes, except in a few places such as the abdominal plexuses where it develops, proliferating all around the aorta and showing the cytological differentiation into chromaffin elements. The chromaffin tissue would thus have a purely mesenchymal origin.

In 1915 and 1917 C. Da Costa studied very carefully the histogenesis of the adrenals and of the chromaffin tissue in the cat and in a few species of Cheiroptera, describing at the same time the development of the sympathetic nervous system. He found the first trace of this system in a 5 mm. embryo of *Myotis* or of *Vesperugo*. These anlagen were formed by a few cellular elements located at the dorsolateral angles of the aorta and were differentiated very easily in the surrounding mesenchyme by a slightly greater stain avidity. There were yet no communicating rami, though in an embryo of *Miniopterus* of 4.5 mm. the sympathetic anlage was near enough to the spinal nerves to suggest its origin from these nerves. The first sympathetic cord is thus found without any connection with the central nervous system, and this author supports the description of Paterson, Fusari and Bruni, suggesting, however, the possibility of an earlier migration. The connection with the central nervous system by the way of the communicating rami appears first in the thoracic and abdominal regions. The cervical part remains free a much longer time from those connections, though its moniliform aspect reveals its segmentary origin. He shows very clearly the derivation of the chromaffin cells from the sympathetic ele-

ments and suggests the name of protosympathetic cells to designate the sympathetic elements before their differentiation either into true ganglionic cells, chromaffin cells or sheath cells. In the 5 mm. embryo of *Myotis* the sympathetic system is represented by cellular strands, composed of only one cellular type with deeply stained cytoplasm and a round richly chromatic nucleus. In a 7 mm. embryo two cellular types can be easily distinguished in the sympathetic anlagen. One of them shows the characters described at the preceding stage, while the other one shows an elliptical nucleus with poorly represented chromatin. The second type characterizes the chromaffin cells. Although no clear transitions could be found between the two cellular types, these careful observations support the theory of the sympathetic origin of the chromaffin cells. As far as the primary origin of the sympathetic system is concerned, the author leaves the question unanswered, requiring further study.

W. Allen ('19) shows that in the embryonic life the motor and sensory fibers of all the spinal nerves of *Polistotrema* and *Squalus* pass to the periphery as separate motor and sensory rami, each ramus possessing a ventral and a dorsal root. He agrees with Onodi ('86) that the prevertebral sympathetic ganglion cells of *Squalus* are derived entirely from neural crest cells which have migrated along the ventral root of the sensory ramus at a time when both roots of this ramus are completely separated from the corresponding motor rami. The few cells which pass out of the neural tube with the ventral root fibers are apparently only concerned with the formation of neurilemma.

Steward ('20) published a beautiful piece of work concerning the cranial sympathetic ganglia in the rat. In an embryo thirteen days old the existence of the superior cervical ganglion and of the carotid nerve is not yet constantly found. Twelve hours later the cervical sympathetic ganglia show a clear tendency to fusion and to a forward extension along the internal carotid artery to a region just dorsal to the pharynx at the level of the second pharyngeal pouch. At fourteen days unipolar neuroblasts migrate out of the superior cervical sympathetic ganglion and insinuate between the fibers of the carotid nerve to join the great superficial petrosal. The vidian nerve may be followed into the sphenopalatin ganglion and with it apparently run an important number of fibers from the superior cervical ganglion. In a sixteen days old embryo the carotid nerve can be traced as far as the carotid canal, with ganglion cells scattered throughout its length. At seventeen days the internal carotid nerve after giving a branch to the hypophysis fuses intimately with the abducens; the latter, soon after this fusion, gives a small anastomotic branch to the trigeminal nerve and numerous ganglion cells are found along it. As far as concerns the vagus portion of the sympathetic, Steward is unable to trace the ultimate origin of the migrating indifferent cells found along it. He emphasizes the real difficulty of determining whether they arise from the neural crest or from the neural tube. He describes numerous neuroblasts along the vagus nerve but never found a direct continuity between them and the vagus ganglia. Migrating cells were found as far as the cardiac plexus. At fifteen days and six hours, numerous neuroblasts are found in the gastric and intestinal plexuses. As regards their origin the addition of elements from the trunk sympathetic system must be excluded, since there is no indication of connections between the coeliac sympathetic and the vagus fibers. The former at this stage do

not yet enter the gastric or intestinal territory. In the vagus trunks two types of neuroblasts are to be found. The first cellular type, of large bipolar cells, is mostly found in the cervical region and becomes very rare below the origin of the recurrent nerve; the second type, of much smaller cells, identical to the true sympathetic neuroblasts, is particularly represented in the pulmonary, cardiac, gastric and intestinal branches.

In 1918 Erik Müller and Goran Liljestrand studied physiologically the autonomic nervous system of rays and sharks. One of the outstanding results they obtained was that the main motor pathway to the intestinal tract was through the ventral roots of the trunk spinal nerves. Since the sympathetic ganglia seem to arise from the spinal ganglia of the dorsal roots, they thought necessary to carry out new anatomical and embryological studies on that point.

In *Squalus acanthias* Müller ('20) finds the first primordia of the sympathetic system in 14 to 17 mm. embryos. Both ventral and dorsal roots present a central fibrillar aspect surrounded by numerous cellular elements which are perhaps more numerous even along the ventral root than along the dorsal one. Those cells, without any doubt, originate from the spinal cord, from the ventral root elements and from the spinal ganglion of the dorsal. At 17 mm. a very clear differentiation is to be observed, the migrating cells along the ventral roots become flat and can not be stained by Bielchowsky's method, while those originating from the spinal ganglia keep their round aspect and are more and more intensely stained with the same technique. At this stage the roots do not show any connection with each other. Immediately after the end of the dorsal root swells into a small ganglionic mass representing the first anlagen of the sympathetic nervous system, of which the origin is doubtless exclusively in the cells migrating from the spinal ganglia along the sensory roots. Still no fusion can be found between the two roots. In 24 mm. embryos the sympathetic ganglia are still in very intimate connection with the dorsal roots. Although both roots are still entirely distinct from one another, fibers from both of them are penetrating into the sympathetic ganglion where they terminate in typical club-shaped endings. Though exclusively derived from the dorsal root ganglia, the sympathetic ganglia receive fibers from both roots. The further development does not offer any particularly interesting point, although Hoffman's statements, that no sympathetic chains are found in selachians and that the sympathetic ganglia corresponding to the ten first spinal nerves degenerate, are proved to be erroneous.

In his description of the vagus ganglion of the selachians, Landacre ('16) was unable to find any structural differentiation between the cellular territories of the ganglion. Müller ('20) distinguishes very striking differences between them. In 26 mm. embryos, the dorsal part of the ganglion contains big, round, regularly bipolar elements and gives origin to the ramus lateralis vagi. The motor medullary nucleus is well defined and from it arises a group of fibers going to the medial part of the ganglion. This latter is formed by smaller cells, among which some show a very rich neurofibrillar distribution, while others are stainless and show small prolongations or are apolar. Small fibrocellular branches grow out of the ganglion, penetrating soon after into the walls of the digestive tract where they present the aspect of a network. At 35 mm. the entire stomach is surrounded by the vagal plexus. The cellular elements found along these branches show the structural characters

of the cells of the vagus nucleus. Their migration from the ganglion is particularly marked in 30 mm. embryos, while in 32 mm. embryos the big trunks near their origin become fibrillar, the cells migrating more peripherally in the small branches. In 35 to 38 mm. embryos along the vagus fibers are found, besides the round isolated neuroblasts, cellular elements containing two or more nuclei, in which the nervous substance is so deeply stained and fills the cell body so entirely that no fibrils can be detailed in their cytoplasm. Most of this vagal network is located inside the external muscular layer, though nerve cells and fibers are found all over the musculature as well as in the mucous membrane. Some have their endings just beneath the epithelium and look like real sensory cells. From the gastric territory numerous fibers pass directly into the walls of the small intestine.

In a later work, Müller ('21) describes in the digestive tract of the selachians two types of nerve cells connected by numerous anastomoses and forming a real network. The first type, existing between the two muscular layers, in the muscularis mucosae and in the mucous layer, is characterized by round, oval or polyhedric cells with a well developed internal neurofibrillar structure. The second cellular type, almost localized in the mucous layer, consists of flattened, ramified cells, generally isolated and possessing poorly developed neurofibrils. The study of their embryonic development shows that the first cellular type originates from vagus ganglion cells migrating along the growing vagus trunk toward the stomach and the intestine; the second type is derived from the trunk sympathetic system. In a five day chick embryo the vagus nerves can be followed to the stomach, the rest of the digestive tract presenting as the only nervous structure the nerve of Remak originating from the pelvic sympathetic. Somewhat later the vagus territory extends to just below the hepatic duct. There is at that stage no migration of cells and no outgrowth of fibers from Remak's nerve. The upper part of the small intestine below the hepatic canal is entirely lacking in nerve elements. In the vagus territory the neuroblasts are grouped in small collections. They possess a clear neurofibrillar structure and show the cytological characters of the cellular type already described in selachians. The region belonging to the nerve of Remak does not show a migration of neuroblasts and nerve fibers in the intestinal wall until the seventh day of incubation and the migrating cells correspond to type II. On the eighth day of incubation it becomes possible to distinguish the Auerbach and Meisner plexuses in the aspect of distinct networks from which branches can be followed to Remak's nerve. These sympathetic networks are entirely different from those characterizing the vagus territory. The fibers are rare, the cells closely disposed near one another, while in the vagus plexuses the neuroblasts are isolated by a rich fibrillar structure.

In 1893 Cajal described in the intestinal wall of frogs and of various mammals asteroid cells with four or more long prolongations which seldom divide. While in those cells Dogiel distinguishes numerous dendrites and one neurite, Cajal and Müller admit that all the prolongations are of the same type. Besides those asteroid cells Cajal noticed the presence of fusiform or triangular small cells with many branches dividing frequently at right angles: they are free neurones existing with real frequency in all layers of the intestinal wall.

Müller ('94) using the Golgi method finds in the intestinal wall and in the pancreas small ramified cells with numerous

identical prolongations. Dogiel one year later shows beautiful pictures of these interstitial cells, of which the branches seem to form real nets identical with those found in invertebrates. Dogiel and Kölliker believe those elements to be specialized connective tissue cells. La Villa with the methylene blue technique admits their real nervous nature on account of their avidity for the methylene blue, their general morphological characters and the various aspects of their prolongations. Cajal confirms this interpretation and shows that after silver impregnation the neurofibrils of the interstitial cells appear even better developed than in the cells of the Auerbach plexus. Müller too admits the nervous nature of the interstitial cells but corroborates the description of Dogiel in so far as the existence of anastomoses going directly from one cell to another is concerned. Heidenhain and Huber consider the interstitial cells as connective tissue elements. Dogiel ('95–'96) believes that the intestinal nervous system as well as the cerebrospinal nervous system is able to show reflex arcs which take place between the sensory cells of type II and the type I exciting the muscular layers. This is not necessarily true, for first of all, many authors do not see any difference between the numerous prolongations of these cells. Furthermore, no synaptic connections have ever been described.

In the higher vertebrates we have no longer the diffuse intestinal nervous system found in selachians but besides the connecting neurones we have interstitial cells. The result of such a disposition is that the conduction of stimuli is no longer reversible but is always similar to the axone reflex of Langley. The two cellular types found in the intestine must have, as in the rest of the autonomic nervous system, antagonistic functions. The vagus cells must be motor, the sympathetic cells inhibitory. It is, furthermore, well known that in the intestine any excitation produces a contraction at that level with a concurrent inhibition of the musculature just below. The vagus motor elements must be disposed so as to conduct the stimulation a short way, whereas the sympathetic neurones with their long prolongations and their arrangement in long series of cells are able to convey the inhibition farther away. In the stomach on the contrary such inhibitions do not take place. We observe a contraction wave, called catastalsis by Cannon, followed by an inverse contraction, the anastalsis. This physiological observation is in complete agreement with the fact that we have here an almost pure vagal plexus. The small inhibitions observed by Cannon after stimulation of the stomach might correspond to the few sympathetic cells in the musculoris and in the mucous layer.

Froriep ('07) was the only man who tried to apply the physiological conceptions of Langley and of Gaskell to the facts shown by embryological observation. He felt that the nerve fibers of the efferent visceral components grew along the ventral roots; the cell bodies remaining in the spinal cord or following the growing fibers to give rise to sympathetic ganglia. The observations of Müller modified this scheme in showing that the prevertebral sympathetic ganglia arise exclusively from the spinal ganglia and that the preganglionic fibers of the efferent visceral elements grow only secondarily in them; the cell body remaining in the spinal cord.

The enteric system shows a more complicated organization and Langley considered it as a special part of the autonomic system, with a particular cellular morphology, a very intricate connection with the central nervous system and an atypical way of reacting to the testing drugs. Gaskell, to answer that question, admitted

that in the Auerbach and Meissner plexuses the vagus fibers originating from cells lying in the central nervous system show their endings. Those preganglionic fibers are in connection with vagal ganglionic cells from which the postganglionic fibers pass to the musculature. On the other hand the sympathetic inhibitory fibers come directly from the prevertebral sympathetic ganglia to the musculature without having a local relay. This scheme does not consider the existence of any sympathetic cells in the intestinal wall and this fact is now definitely shown. Müller admits that the intestinal nervous system differs from the general autonomic system in presenting a third relay by way of the migrating cells into intestinal walls. The sympathetic and vagal systems are in this way identical. The vagus ganglion corresponds to a vertebral spinal ganglion, the visceral part of which has remained within the nucleus of origin, while the visceral part of a spinal ganglion isolates itself in forming the prevertebral sympathetic ganglion. The intestinal neuroblasts are the third relay both for the vagal and the sympathetic systems.

Müller and Ingvar ('21) studied very carefully the development of the sympathetic system in amphibians. They confirm the description given by previous authors, especially by Held ('09), concerning the ectodermic origin of the sympathetic nervous system, its cellular elements being migrating cells from the spinal ganglia. They were able to carry out the famous experiments of removing the dorsal half of the spinal cord in young embryonic stages, obtaining in that way a total absence of sympathetic anlagen. Their embryos did not live more than fourteen days, some of them entirely lacking a trunk sympathetic system.

The same authors using Bielchowsky's technique were unable to find any isolated sympathetic cells in the prevertebral mesenchyme of chick embryos. They believe that the cellular elements described by Cajal at 52 hours and by Held at 60 hours might be connective tissue cells possessing an argentophile intracellular reticulum. In destroying the ganglion crest of young embryos by means of electrocauterization, they produce the absence of sympathetic anlagen in the injured segments. The ganglion crest seems to be the only source of sympathetic elements. The two series of normal morphological observations are strikingly confirmed by the experimental results and they constitute strong evidence in favor of the theory that the origin of the sympathetic primordia is from the neural crest by migration of its elements along the dorsal roots. Kuntz and Batson ('20) also made an experimental study on the same groups, but obtained exactly inverse results. We shall not discuss this work of Kuntz at present since we are going to consider his very important contribution as a whole.

In a very extensive and careful piece of work, S. Rau and P. Johnson ('23) studied the development of the sympathetic nervous system and of the suprarenal bodies in the sparrow. Taking into special account each one of the three possible origins of sympathetic elements, the neural crest, the dorsal root ganglion and the ventral root, they hold that the sympathetic primordia are derived from three sources and their drawings try to illustrate this. The neural crest contribution is to be seen in embryos of five to seven somites, corresponding to a 32-hour chick embryo. The crest at this moment reaches the ventral edge of the spinal cord and numerous cells are wandering into the mesenchyme between the neural tube and the aorta. These isolated cells show the cytological characters of the crest and are very clearly distinguishable from the mesenchymal

cells. In a somewhat older embryo corresponding to a 42 hour chick, true neuroblasts can be seen migrating from the spinal root ganglion toward the dorsolateral angles of the aorta. Still later some cellular migration can be seen from the ventral half of the spinal cord along the ventral roots to the same aspect of the aorta. While it appears that many of these migrating cells are sympathetic in nature, the possibility must not be excluded that some of them will develop into the sheath cells. In the early stages, the sympathetic primordia are arranged metamerically and almost located in the intersomitic regions. Later stages show the growth of longitudinal cellular strands uniting the dispersed cell masses into two parallel continuous moniliform chains, of which the authors modeled very careful reconstructions. In later stages the primary ganglionic swellings of these cords send dorsolateral extensions, the free ends of which enlarge and, becoming separated from the primary ganglia, give rise to the definitive sympathetic ganglia and to the secondary sympathetic chain. As far as concerns the communicating rami there has arisen a considerable amount of confusion owing to the fact that there exist three sets of connections between the sympathetic primordia and the central nervous system: the cellular strands giving rise to the primary cords, the dorsal extensions of those to form the secondary chains and the permanent connections between the secondary sympathetic ganglia and the nerves. Ganfini ('16) goes so far as to use the terms primary, secondary, and tertiary rami. The two first communicating branches have only a transient existence except for a possible rôle in the constitution of some visceral plexuses. The contributions of the sympathetic system to the suprarenals all arise from the primary sympathetic cords at the very moment when they are sending their dorsal extensions to give rise to the secondary chains.

The triple derivation suggested by Rau and Johnson represents the most conciliatory idea that will bring agreement among the different investigators. One can not help doubting the soundness of this attempt. The neural crest contribution does not seem to be a very important one. The migrating cells along the ventral roots might just as well represent sheath cells. The dorsal root ganglion origin is the only definite one and anyhow is by far the most richly represented. Their idea about a possible rôle of the primary chains in the formation of visceral plexuses has found a very striking confirmation in a recent work of Willier ('18).

Goormaghtigh ('14–'21) studied the development of the coeliac plexus and of the adrenals in the chick embryo. He considered the sympathetic chains as originating from true ectoblastic ganglionic cells, while the chromaffin elements were mesodermic in origin. In 1924 he modified entirely his conception. In a 50 hour chick embryo, cellular bands are migrating from the dorsal part of the nervous system toward the dorsolateral angles of the aorta. They represent the neural crest. This neural crest differentiates into three segments: the dorsal one gives rise to the spinal ganglia, the middle one is the source of the Schwann cells, and the ventral one contributes to the formation of the primary sympathetic chains. The secondary or permanent sympathetic cords are formed by migration of medullary elements along the ventral roots at the stage of 73 to 78 hours of incubation. At the same time the primary chain regresses, its elements acquire a glandular structure, numerous blood vessels penetrate into the cellular mass. These transformations represent the differentiation of the chromaffin tissue. The chromaffin elements are derived

from the neural crest, while the sympathetic ganglion cells are wandering out of the neural tube along the ventral roots. It is possible that the sheath cells of the visceral nerves are derived from the elements of the regressing primary chains.

F. Tello ('25) studied the histogenesis of the sympathetic nervous system in chick embryos using the silver impregnation method. The first traces of the primary sympathetic chain appear in the second half of the third day. At the posterior aspect of the aorta he describes a few isolated cells showing a delicate neurofibrillar differentiation. They are entirely independent of the spinal mixed nerves, which pass along the internal border of the myotone. The silver impregnation and the neurofibrillar structures are very clearly marked and there can be no doubt about the nervous nature of those elements. Progressively the sympathetic primordia concentrate around the point of origin of the intersegmental vessels and the prevertebral primary sympathetic cords are formed. At the beginning of the fifth day the sympathetic neuroblasts of this continuous chain show a dorsipetal migration toward the point of junction of the anterior and posterior spinal roots. The most dorsally situated elements show numerous mitotic figures, giving rise to cells which migrate forward or backward in order to connect with the elements of the preceding or following segment. Around the sixth day the secondary sympathetic chains are constituted. They are located very near the fusion of both spinal roots and they soon acquire definite connections with them through communicating rami. Thus Tello admits the exclusively mesodermal origin of the primary sympathetic primordia. They immediately concentrate into a continuous bilateral chain. The permanent sympathetic chains are entirely derived from the migrating elements of the primary ones and are not originated from the central nervous system.

Taking into special account the structure and development of the gastrointestinal plexuses, Tello describes in the three-days old chick embryo isolated fusiform elements at the level of origin of the hepatic and pancreatic diverticula. These cells send off numerous anastomitic branches, giving rise to a well defined gastroduodenal plexus extending upon the stomach and the anterior part of the small intestine and which in the following days mingles its fibers with the terminal fibers of the vagus nerve. The posterior intestine, from the end of the duodenum to the cloaca, is innervated by the ganglionated nerve of Remak originating from the aortic plexus and penetrating into the intestinal wall by its juxtacloacal extremity. In 4 mm. long mice embryos, the stomach and the anterior part of the intestine show a very rich layer of nervous elements between the entoderm and the splanchnopleural sheet. At that time the vagus nerve ends in the very upper part of the stomach, the splanchnic nerves are not yet formed and the visceral branches of the spinal nerves do not reach the ventral aspect of the aorta. The neuroblasts just described have necessarily a local, mesodermal origin. In 10 mm. embryos the submucous plexus, exclusively fibrillar, sends numerous fibers ramifying between those cells. The posterior part of the intestine remains without any innervation until the stage of 12 or 13 mm. when its nervous supply arises by a very small extension from the anterior intestine and from the superior and inferior mesenteric plexuses. In the anterior intestine the Auerbach plexus is alone present in the stage of 15 mm. At this time the mesenteric plexuses have appeared and in the intestinal wall the smooth muscle layer and the plexus of Meissner make their appearance. Weber

('51) described a human foetus of about full term in which brain and spinal cord were entirely absent, although the peripheral nerves and the sympathetic nervous system were normally developed. Dart and Shellshear ('21) have interpreted this and other similar cases in which peripheral nervous elements are found in the absence of the cerebrospinal nervous system as being a demonstration of the mesodermal origin of the peripheral nervous system including the sympathetic system. Lehman ('27) also considers those cases as supporting his theory of the mesodermal origin of the spinal ganglia. It is most probable that such an interpretation does not correspond to the facts and that in these cases the central nervous system must have undergone degeneration after the peripheral system had arisen. Furthermore, a few contradictory cases were reported, especially by Alessandrini (1829), who describes a calf born with an absence of spinal cord in a few thoracic segments and in which the spinal nerves and sympathetic trunks were absent in the corresponding region. Those cases afford no evidence of value regarding the origin of the sympathetic system, since the previous history is entirely unknown.

In various publications Ganfini ('08, '12, '14, '16) studied the development of the sympathetic nervous system in numerous groups of vertebrates. His careful observations represent a new attempt to admit a mixed origin of the sympathetic anlagen instead of an exclusive derivation from one or another part of the central nervous system. In *Amia calva* he finds the primordia of the sympathetic system in 6 mm. embryos. The spinal nerves are already completely formed and numerous neurocytes are found along the ventral as well as along the dorsal root. In 12 mm. embryos true sympathetic ganglia are well isolated in the surrounding mesenchyme. Soon after, they connect with one another, forming a continuous bilateral chain. The two chains unite at the level of the eighteenth spinal nerve, forming there the preaortic sympathetic commissure. In 15 mm. embryos the sympathetic chains extend cranialwards to the acousticofacial ganglion; the cellular differentiation is nearly complete and true ganglionic cells are found. At 20 mm. he distinguishes four regions. In the cranial region the sympathetic shows a plexiform aspect surrounding the aortic and carotid vessels, being close to the vagus nerve and the first spinal nerve without showing any connections with them. In the first five spinal segments the sympathetic ganglia are disposed in regular correspondence with the communicating rami, numerous small efferent branches growing toward the aorta. From the sixth to the thirtieth spinal nerve there exists a rich plexiform sympathetic system with frequent fibrillar anastomoses. In the fourth region the ganglia are regularly arranged. The intestinal nervous system is a direct outgrowth of the sympathetic chains. The motor root when alone, is still able to differentiate a sympathetic ganglion. In two occipital nerves he found the absence of spinal ganglia and of dorsal roots and in spite of that the existence of clusters of neurocytes at the angle of the carotid.

In 5.5 mm. embryos of *Chrysemys marginata* the first twelve spinal nerves are present and both roots are formed by big round cells showing many mitotic figures. The cells of the ventral root are doubtless medullary cells migrating directly along the motor fibers, while those found along the dorsal root arise from spinal ganglion cells. At the ventral border of the myotome the motor and sensory neurocytes group into cellular clusters which are the primordia of the sympathetic ganglia; no cranial sympathetic can be found yet. At

6 mm. the spinal nerves become fibrillar but numerous neurocytes are still migrating along them. These neurocytes keep their round aspect while others look more and more elongated so as to become sheath cells. At the ventral border of the myotome the neurocytes migrate toward the lateral angles of the aorta where they form the sympathetic ganglia connected with the mixed spinal nerve by the communicating ramus. The first three spino-occipital nerves have no dorsal roots, having only a small anterior root with a rudimentary sympathetic cluster. At the stage of 7 mm. the communicating rami of the last two spino-occipital nerves and those of the first seven spinal nerves are mostly atrophied. From the eighth to the fifteenth spinal segment, besides the ramus existing at the preceeding stage, a new communicating branch has appeared, purely cellular, situated more dorsally than the primary one, nearly parallel to it but entirely independent. The ciliary ganglion has developed by cellular elements migrating along the ophthalmic branch of the trigeminal nerve and by others following the fibers of the oculomotor nerve. In embryos 9 to 9.5 mm. the trunk sympathetic system is almost identical with that found in the preceding stage. The peripheral part of it is much more developed, the sympathetic trunks being followed very near to the anlagen of the interrenal organ. Between this stage and the 10 mm. one, the sympathetic chains have grown very extensively in the cranial and caudal directions. Cranially it has acquired very intimate relations with the vagus nerve and at the level of the first spinal nerve it shows a direct anastomotic branch to the vagal ganglion, which might suggest an exchange of cellular elements between the two trunks. The first seven spinal nerves do not show any trace of communicating ramus. From the eighth segment on, besides the secondary ramus a tertiary one has appeared. It is a very short branch, at once fibrillar, terminating in a new cellular heap, the secondary or vertebral sympathetic ganglion, distinct from the primary or aortic ganglion. The secondary sympathetic ganglia soon become united to each other by commissural branches and to the corresponding primitive ganglion by a small fibrocellular tract. The secondary or vertebral sympathetic ganglia are derived from migratory neurocytes of the corresponding spinal ganglion. They appear as direct extensions of the spinal ganglia and no contribution from the spinal cord by way of the ventral roots is to be found.

In the development of the sympathetic system in birds, a few details are worth mentioning. In chick embryos from three to four days of incubation, the mixed spinal nerves are already formed, the anterior root being mostly fibrillar, the posterior one cellular. At the basal part of the anterior root numerous neurocytes, doubtless originating from the medullary tube, are grouped into a small cellular cluster from which cells detach themselves to migrate along the motor fibers and to mingle with the migrating elements derived from the spinal ganglia. The primary sympathetic chain disappears and as in reptiles is followed by the differentiation of a secondary one. Soon after this moment, the peripheral sympathetic nervous system is formed by the growth of periaortic fibrocellular trunks, of which branches are going to the kidneys, adrenals, gonads and anterior part of the intestine. The posterior part of the intestine is innervated by the nerve of Remak, itself originating from the periaortic plexus at the level of the gonads and of the adrenals. From the cervical part of the primary sympathetic chain a few branches anastomose with the branchial ganglion of the

vagus nerve, and they very probably represent the source of the sympathetic elements which by the way of the vagus nerves migrate toward the lungs, the heart and the oesophagus. The cranial sympathetic system is represented by the ciliary ganglion and the cranial extension of the primary and secondary trunk sympathetic chains. The superior cervical sympathetic ganglion is located near the glossopharyngeal and vagal ganglia, with both of which it is connected by anastomotic branches.

In mammals also Ganfini ('18) describes the double origin of the sympathetic ganglia, from medullary elements migrating along the motor roots and from spinal ganglion cells. In the cervical region the communicating rami are purely cellular. They disappear very quickly, and the cervical sympathetic chain remains without any direct connection with the cervical spinal cord. In the thoracoabdominal region the primary sympathetic chain is first lateral to the aorta, becoming ventral to it at a later stage. At the moment of that change in position, new neurocytes accumulate in a longitudinal cord located at the dorsolateral angle of the aorta, representing a true secondary chain. The two chains are very near each other, in some places in direct continuity. When they have received their total supply of migrating neurocytes, the primary cellular communicating rami are replaced by new fibrillar connecting branches to the secondary sympathetic chain, a small twig remaining in direct relation with the primary ganglion which has migrated, ventrally. Ganfini is inclined to admit that while the primary sympathetic ganglia contain elements migrating along both roots, the secondary ones derive exclusively from cells of the spinal ganglia.

These careful observations of Ganfini are extremely interesting, showing that a common scheme may be applied to the development of the sympathetic chains in all the series of vertebrates, except the selachians. The chronological succession of two generations of sympathetic elements is found in all groups with some variation concerning the details of their arrangement. The double origin of the primary elements, as opposed to the spinal ganglion origin of the secondary ones, is an entirely new idea, the importance of which is not to be lessened and which might explain a goodly number of divergent observations. One might regret that the earliest stages studied by Ganfini in reptiles, birds and mammals were already too old as far as the primary origin of the sympathetic elements is concerned. His observations do not eliminate the possibility that the cells found along the ventral roots may derive from the spinal ganglia or even from the neural crest. We know that in the development of the chick the stage of three days and twelve hours is already a very old stage as far as the primary sympathetic chain is concerned.

We have now to review the work of A. Kuntz, whose very numerous publications dealing with the sympathetic nervous system in various series of vertebrates represent the most extensive investigations ever carried on in this field from the embryological standpoint. We shall follow the chronological order and shall take each paper as basis for a comparative discussion of the numerous observations hitherto reported.

In 1909 he studied the rôle of the vagus nerve in the development of the sympathetic nervous system in pig embryos. This paper is in fact the basis of Kuntz's theory and deserves some attention. In a 6–7 mm. pig embryo the vagus nerve does not extend beyond the level of the heart. In the walls of the stomach the paths of the vagus branches are indicated by the pres-

ence of numerous cells which are easily distinguished among the mesenchymal cells by their larger size and the characteristic chromatic structure of their nuclei. These cells show a tendency to spread into the entire gastric wall until they have completely surrounded the stomach. Similar cells are found scattered in the walls of the intestine throughout its length as if, once having reached the anterior region of the digestive tract, they were able to migrate posteriorly all along its course. That those migrant cells found in the intestinal wall have wandered out from the vagus trunks can not be doubted. It is impossible to trace them to any other sources. There is no evidence of ventral migration of cells from the ganglia of the sympathetic trunks in the regions of the oesophagus or of the mesentery, except the fact that in the regions where later the coeliac and hypogastric plexuses develop a few sympathetic cells can be traced to the ventral aspect of the aorta. At 12 mm. the vagus branches can be followed to the lesser gastric curvature. There exist neither cellular nor fibrillar connections between the sympathetic trunks and the intestinal wall. These connections do not appear before the stage of 16 mm. and not until then do numerous sympathetic cells wander down into the myenteric and submucous plexuses. The anlagen of the pulmonary and cardiac plexuses originate evidently from cells wandering from the vagus trunks. The coeliac, renal and hypogastric plexuses arise from the sympathetic trunks and are easily seen in 10 mm. embryos, forming somewhat continuous fibrocellular masses. As far as concerns the primary origin of the elements forming the sympathetic anlagen, he describes a cellular migration into the ventral roots, with a later contribution from the spinal ganglia along the dorsal roots. This description is in fact the review of three different papers published the same year and represents the type of Kuntz's descriptions of the histogenesis of the autonomic nervous system in mammals.

It might be remarked that in a 6–7 mm. embryo in which the vagus trunk is still behind the heart, Kuntz describes vagal-sympathetic cells not only in the stomach but also in the intestinal wall and that he accepts their vagal origin mostly because he does not see any other origin. Since those cells do not present any direct continuity with either the vagus or the sympathetic trunks, we do not see any serious reason to admit the first origin rather than the second one. A mesodermal origin, in the conception of Tello, might just as well be considered. Concerning the primary origin of the sympathetic elements, no definite conclusion could be drawn as to the origin and destination of the cells found along the ventral roots. Harrison ('01) was one of the first to suggest that the few medullary cells found along the ventral roots of *Salmo* might contribute to the formation of the sympathetic primordia. The many other workers who described those migrating medullary cells did not make a careful distinction between true medullary cells migrating out of the spinal cord and cells found along the motor roots. Cells may be described just outside the external membrane and yet result from a migration towards that region. Kuntz's figures in particular probably deal with this second cellular type, especially since his stage of 6–7 mm. is very evidently too old. The mixed nerves are formed and covered with sheath cells. The sympathetic elements have already migrated near the dorsolateral angle of the aorta.

Kuntz ('10) next investigated the development of the sympathetic system in birds. He verified the old description by His of the succession of two sympathetic

chains, the primary system existing from the fourth to the sixth day; the permanent one beginning its development during the sixth day. The prevertebral plexuses originate entirely from the primary sympathetic trunks. They are already clearly defined at the end of the fourth day with a direct cellular continuity between them and the primary chain. Sympathetic cells in very great abundance can be followed to the anlagen of the adrenals. At the end of the sixth day the majority of the elements of the primary trunks have migrated into the prevertebral plexuses. From these plexuses, however, neither cells nor fibers extend more ventrally, except in the sacral region, where from the hypogastric plexus numerous cells migrate toward Remak's ganglion; the latter can be observed from the fourth day on. The ganglion of Remak has no corresponding structure in mammals, but in reptiles, especially in turtle embryos, numerous ganglionic masses can be observed dorsally to the rectum, representing doubtless the prototype of Remak's ganglion. In a 130 hour chick embryo the vagus trunks are found along the walls of the oesophagus, the vagal ganglia being located very near its lateral walls. In front of the bifurcation of the trachea, numerous vagal cells wander out into the oesophageal wall, being very poorly differentiated and difficult to distinguish from the surrounding elements. At the end of the sixth day those migrating cells have formed a double ring around the oesophagus. The plexuses in the intestinal region appear only during the seventh and the eighth days, but Kuntz was unable to observe their origin clearly. There is no evidence of any penetration of cells from the sympathetic trunks or the prevertebral plexuses. Many cells probably arise from Remak's ganglion by a forward migration. Still he considers that a posterior migration of vagus cells must represent the most important contribution. This last conclusion looks rather an audacious one and is not supported by any valid argument. To recall an analogy with histogenesis in mammals is far from bringing any evidence.

The histogenesis of the sympathetic nervous system in amphibians was studied on embryos of frog and of *Amblystoma* ('11). The sympathetic elements in this group obviously wander from the spinal ganglia or perhaps from the neural crest. The cells can easily be traced from the distal end of the spinal ganglion to the lateral aspect of the aorta. There is very little evidence of any migration of medullary cells in the ventral nerve roots, and Kuntz establishes a correlation between this fact and the small amount of sympathetic elements existing in embryos of this group. The myenteric and submucous plexuses are very difficult to recognize and their development could not be traced with great accuracy. In spite of this fact Kuntz considers that his preparations leave no doubt as to the genetic relationship of these plexuses to the vagus nerves and that no other origin could be traced.

The study of fishes ('11) shows very interesting details. In 6 mm. embryos of *Acanthias* the spinal ganglia are not yet differentiated. The neural crest cells may be observed along the lateral aspects of the neural tube. Numerous medullary cells are pushing out of the cord into the growing ventral roots. At 7.8 mm. in length the neural crest cells migrate beyond the ventral level of the neural tube, while numerous medullary cells still follow the ventral root fibers. The cells of both sources become scattered in the mesenchyme and show a tendency to concentrate at the dorsolateral angle of the aorta. It is very easy to distinguish the two cellular types. The neural crest cells have a much larger nucleus than the med-

ullary cells. At 10 mm. the spinal ganglia are definitely differentiated, and from their distal poles numerous elements are migrating along the dorsal root. The motor roots have become fibrous, although a few medullary cells can still be observed pushing out of the neural tube. The migration in the dorsal roots goes on for a considerable period of time; the wandering cells being still scattered in the retro-aortic mesenchyme. The aggregation of these elements takes place only at the stage of 13 mm., but at this moment the motor and sensory bundles are not fused but situated near each other, so that the sympathetic primordia show connections only with the sensory root. Neither the spinal ganglia nor the neural crest may be looked upon as the sole source of the elements contributing to the formation of the sympathetic system. An important part doubtless originates in the ventral half of the neural tube. The wandering of sympathetic cells seems to be the result of the attractive action of hormones which are produced by the cellular elements in the highly vascularized regions. This could explain the migration toward the aortic region and toward the cardinal veins. As far as concerns the myenteric and submucous plexuses no clear figures of their development could be observed, owing to the fact that the intestinal wall becomes muscular before the development of those plexuses. He feels that the vagal origin is the only possible one.

In a 4 mm. embryo of *Amia calva* numerous cells migrate ventralward from the spinal ganglia, while there exist no clear pictures of medullary cells wandering along the ventral roots. Migrating cells are only found at 8 mm.; it might be recalled that Ganfini described them at the stage of 6 mm. The selachian material has been studied by Müller, and we have seen the quite different interpretation given by him.

In his other papers dealing with the development of the sympathetic system in man ('20), in the turtle ('11), or with general applications to the series of vertebrates ('21), Kuntz does not add any new facts. The same theories are presented and the same critical considerations apply to all of them.

The first experimental attempt was realized by Kuntz and Batson ('20) on chick embryos. In seven chick embryos of 48 hours of incubation, they destroyed the dorsal half of the nervous system in a few segments, by the electrocautery method. In spite of this destruction, four or five days after operation they found a normal disposition of the sympathetic trunks as well in the injured region as in the remaining part of the embryo. Their conclusion was that the migration of the sympathetic cells from the neural tube seems to be accomplished along the ventral spinal roots.

Kuntz ('22) published another paper based on experimental researches in chick and frog embryos. The chick material is to a great extent the same as he described in the first publication, and we shall not discuss it for the moment. Various operations were performed on frog embryos, such as removal of the neural crest and dorsal half of the nervous tube, extirpation of the entire spinal cord and removal of the medulla. The animals were allowed to develop isolated, or two animals were healed together. They all show a markedly slowed development. Complete removal of the spinal cord prevents the development of the sympathetic anlagen. These are thus genetically related to the cerebrospinal nervous system, as is admitted by most workers. Embryos from which a strip of tissue including the neural

crest and the dorsal half of the neural tube was removed, show three or five days later a complete absence of spinal ganglia and dorsal nerve roots, while the ventral roots accompanied by medullary cells and the primordia of the sympathetic trunks are present in all segments providing an important amount of nervous tissue is left intact. As far as concerns the visceral plexuses, the frog embryos in which he removed the medulla did not live a sufficient period of time to allow definite conclusions.

Complete removal of the neural tube in chick embryos from the lower cervical to the sacral region prevents the development of the sympathetic trunks and of the prevertebral plexuses in the lumbar region. In the thoracic region the operation was incomplete on one side. In these operated embryos the primordia of the oesophageal, pulmonary, cardiac and enteric plexuses are present and apparently contain as many cellular elements as in unoperated embryos of five days of incubation. Inversely, in one embryo he successfully destroyed the hindbrain. It was found dead five days later. The microscopic study showed normally developed sympathetic trunks and prevertebral plexuses, while the primordia of the pulmonary, cardiac and enteric plexuses were entirely lacking.

This experimental work of Kuntz represents a very interesting attempt to discriminate the real source of the sympathetic elements and the constitution of the visceral plexuses. There are, however, a few objections which might be formulated against his interpretations. Concerning the primary origin of the sympathetic elements in frog embryos his deductions are not at all convincing; his figures are poor and the so-called migrating medullary cells in the ventral roots may just as well be considered as mesenchymal cells. He furthermore did not make a careful search for isolated ganglion cells or Rohon Beard cells. The careful piece of work of Müller and Ingvar, corroborated by the descriptions and the experimental results of Harrison, seems of much greater value. Harrison admits some migration of medullary cells along the ventral roots, but he considers that they probably become sheath cells. They are very few in number, and the experimental evidence of their existence was only observed twice in the experimental series.

If we consider the same question in chick embryos, we are also not convinced of the soundness of Kuntz's conclusion. The operations were always incomplete, the described result being found only in a few segments or on one side. That means the possibility of a direct extension from the adjacent segments or from the other side of the embryo. We do not know the exact morphological stage of development at the time of the operation; the number of hours of incubation is too inaccurate a way of determination on account of the very great variations occurring in a group of eggs placed in the incubator at the same moment. The result of this last consideration is that we do not know whether or not any migration of neural crest material occurred before the operation. Electrocauterization is a blind way of doing experimental embryology; in using this method it is practically impossible to localize the lesion exactly and to determine its extent.

If we consider the second point of Kuntz's theory, the rôle of the vagus nerve in the constitution of the so-called vagosympathetic plexuses, we find only one embryo in which he removed the vagus nerves successfully. Unfortunately the embryo was found dead after a total incubation of five days. It is doubtful whether, according to the morphological description of normal embryos given by

Kuntz himself, this embryo would be old enough to show the enteric plexuses and whether the post mortem alterations as well as the progressive death of the embryo did not make very uncertain the identification of those plexuses.

Taken as a whole, the experimental part of the researches of Kuntz is by no means conclusive. The immense interest of this research is the fact that Kuntz was the first investigator to apply experimental methods to the embryology of the sympathetic nervous system. Müller and Ingvar repeated the same experiments a few years later with quite different results, and we willingly would give more weight to their work because of an apparently cleaner experimental procedure. However, the question remains entirely unanswered and further researches will be needed. Detwiler ('29) throws a doubt on the interpretation of experimental results after the removal of the neural crest. In three Amblystoma embryos in which he removed the neural crest by a complete scraping at the tail bud stage, well developed spinal ganglia were found. He admits a complete regeneration of the neural crest. It would be very important to know what was the dorsal limit of the operations, in order to determine the exact source of the new forming neural crest.

(To be concluded)

LIVING WATER

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INTRODUCTION

WATER is the chief chemical constituent of most living organisms. Being the medium in which the majority of the other constituents live and move, its presence is more often taken for granted than considered as a major regulation by the organism. For living substance is not merely endowed and kept; it is in communication with its surroundings. From its environment it continually or interruptedly takes up new materials, and to its environment it gives up some of what it had. Whether all such exchange is necessary, or whether some is accidental to the preservation of normal functioning, can hardly be decided experimentally.

The vortex of vital activities is with respect to water more than analogous to the whirlpool. Without water both the body and the pool would be but paleontological specimens, showing only (in Huxley's phrase) where activity has been.

In studying the various constituents of living organisms, it is customary to refer their quantities to that of water. Contents are expressed therefore as concentrations. But the content of water can hardly be so expressed. In many investigations water is represented as wet weight minus dry weight. This conveys little of physical or physiological significance, except in the case of direct and immediate comparisons. Somewhat more informing are determinations of total osmotic pressure and its various correlatives. These values are useful because they measure one of the forces which control the exchanges of water among tissues.

RATES OF TURNOVER OF WATER

The most important measurement concerned with water balance is the rate at which water is continually passing into and out of body or tissue. In organisms which have definite paths of assimilation and dissimilation, to measure the rate is a feasible procedure; and some of the results obtained are listed in table 1. Further data exist upon other species (56), particularly of marine fishes (24).

In man practically all assimilation is from the alimentary tract, and all elimination is through the urinary tract, the skin, and the lungs. In the frog all intake is through the skin; the output which can be measured is through the cloaca. To tell how much fluid passes both in and out through the skin is no more possible than to tell how many molecules of water pass through an osmometer only to be instantaneously counterbalanced by an equal number moving in the opposite direction. In the earthworm a still smaller proportion of the total water exchange can be actually measured, namely that which is eliminated through the anus. Finally in *Ameba*, where the volume of contractile vacuoles only is measured, it is likely that only a few per cent of the total exchange of water are actually detected. The values given in table 1 are, therefore, the lower limits of the actual turnovers.

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Comparative rates of excretion have been analysed by Pütter (56) to demonstrate that among various species of animals the rate of water output is inversely proportional to the cube root of the body weight, *i.e.* to the linear dimension. Much more accurate data are required to make this generalization significant.

It is obvious that there is for each species a tremendous range of turnover rates, within which the water content of the organism may remain constant. What is required for normality is that the intake and output shall be equal. Thus, a frog may be taken from water and put in an atmosphere nearly saturated with moisture, when no water will be absorbed, and practically none excreted (12). But the individual tissues are none the less in their normal state. If a man drinks water as fast as his alimentary tract will absorb it, the kidneys will excrete it about as fast as it is absorbed. Yet the dilution of the blood is so small that it can be detected only by a few very precise methods (55).

TABLE 1

Rates of turnover of water, expressed as per cent of body weight per 24 hours, at about 20°C.

SPECIES	MODE	MINIMUM	MAXIMUM	REFERENCE
Man	3.4	1.4	68.0	(1)
Frog	31.0	<5.0	93.0	(12)
Earthworm	19.0	1.0	>30.0	(9)
Ameba	520.0	<75.0	>615.0	(6)

A comparison of the turnover of water with that of other substances shows that in man water occupies a high place. Whereas (table 2) nitrogen is replaced in the human body only every 290 days on the average, water holds its position in the body only twenty-one days. It is most nearly rivalled by sodium chloride; this is due more to dietary habits than to necessity. Obviously, water has this unusually high rate of turnover because it is the substrate, the solvent and conveyor, for all other substances. In birds, where probably less exchange takes place through the skin, and where even urinary water is reabsorbed, water has a relatively low rate of turnover. But in freshwater animals such as frog, earth-worm, and *Ameba*, the turnover of water is very far above that of all other substances combined. In marine animals (24) the rates of water turnover appear to be less than in freshwater ones; in them all intake is probably through the alimentary tract.

TABLE 2

Average rates of turnover of each chemical element in a 70 kilogram man. Most of the data from which this table is drawn up are contained in the book of Sherman (63)

ELEMENT	BODILY CONTENT	DAILY REQUIREMENT	DAILY REQUIREMENT AS PER CENT OF CONTENT
	gm.	gm.	
C	12,600	250.0	2.0
O	45,500	3350.0	7.4
H	7,000	350.0	5.0
N	2,100	7.13	0.34
S	175	0.68	0.39
P	700	0.88	0.126
Ca	1,050	0.45	0.043
K	245	3.40	1.39
Na	105	1.95	1.86
Mg	35	0.34	0.97
Fe	3	0.01	0.33
Cl	105	2.85	2.71

The metabolism of water therefore depends upon its availability in the environment. But it depends also upon a number of other factors, such as the ability of the body to conserve water, as against dehydrating forces; the bulk and surface of the body; and the various phylogenetically determined properties of the body and its tissues.

WATER EXCHANGES OF FROGS

The organism whose exchanges of fluid with the environment have been studied most extensively is the frog. But there are differences among various species of frogs, and the description may therefore be limited first to adult *Rana pipiens*.

The rate of accumulation of urine turns out to be proportional to the bulk of the body, and at 20°C averages 1.3 per cent of the body weight per hour, or 31 per cent per day (12). This amount of fluid is

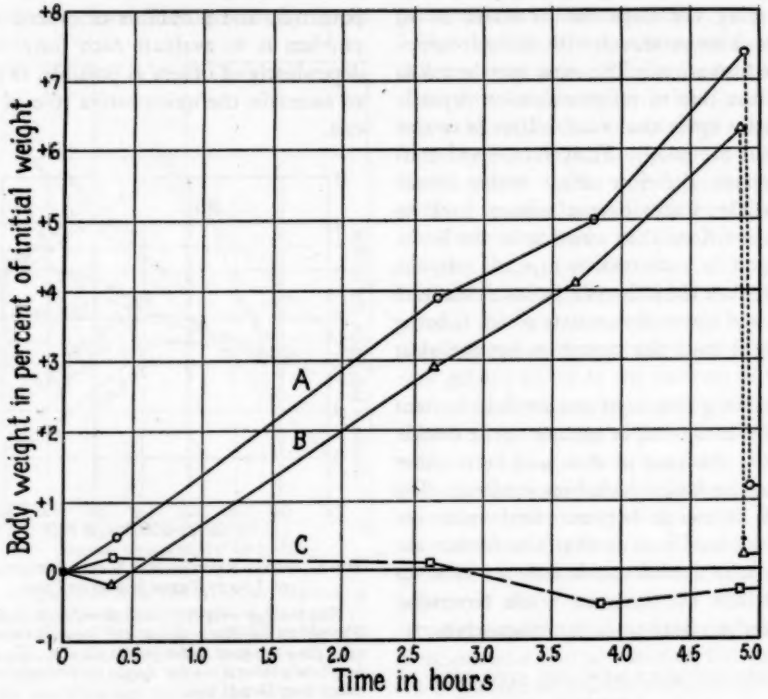

FIG. 1. PROGRESSIONS OF BODY WEIGHT IN 60-GRAM RANA PIPIENS KEPT IMMERSED IN TAP WATER

A and *B* were bound at the cloacae at zero time; *C* was not bound. At about 4.9 hours the cloacae were unbound, allowing the urine which had accumulated in the bladder to escape. The slopes of the lines *A* and *B* therefore are the rates at which water passed inward through the skin.

When immersed in tap water or distilled water, frogs are, of course, in dynamic fluid equilibrium, as is indicated by the fact that body weight is relatively constant. That the equilibrium is dynamic is shown by binding the cloaca with thread, when urine is forced to accumulate in the bladder. The results of such an experiment are indicated in figure 1.

The rate of accumulation of urine turns out to be proportional to the bulk of the body, and at 20°C averages 1.3 per cent of the body weight per hour, or 31 per cent per day (12). This amount of fluid is continually passing in through the skin, for unfed frogs never ingest water by mouth. At any other temperature a balance is likewise struck, though the rate of passage of fluid is increased about 2.3 times for each increase of 10° in temperature.

The rate of fluid turnover may also be

varied by placing frogs in diverse solutions. In various concentrations of sodium chloride up to 0.15 osmolar, the rates of urine formation are equal to that in tap water, but are smaller in higher concentrations, so that above 0.22 osmolar the rates are constant at only 5 per cent of the body weight per 24 hours (12).

Placing the frogs out of water in an atmosphere saturated with moisture stops urine formation. The view may be taken that the rate of water excretion depends directly upon the availability of water within the body. Temperature and concentration therefore affect water intake primarily; water output adjusts itself to the conditions then existing in the body. If water or a solution is injected into the body, then the kidneys eliminate this fluid over and above the amount which is being brought into the organism by the skin (12).

The frog thus regulates its fluid content chiefly at the output and not at the intake. This is the case in man and most other organisms which have been studied. The chief difference between freshwater organisms and man is that the former are ordinarily forced continually to take up fluid from the medium; while terrestrial species can deny or satiate themselves.

FORCES CONCERNED IN WATER EXCHANGES OF FROGS

Experiments with isolated frog skin demonstrate that this continual inward passage of water does not depend entirely upon the fact that the concentration of substances is greater inside the body than outside it. The skin may be reversed in orientation, when the rate of passage of water from the medium to Ringer's solution is many times faster than in the normal orientation (4). An experimental analysis of the forces concerned in the passage of water therefore becomes necessary. Owing to the great irregularities found in measurements of fluid migration through isolated skin, the chief observations must be carried out upon living frogs.

The rate of any movement of water must be proportional to the forces producing it. These forces may include gravity, friction, tissue tension, osmotic pressure, electrical potential, and a number of others. The problem is to evaluate each force as independently of others as possible, in order to ascertain the quantitative rôle of each one.

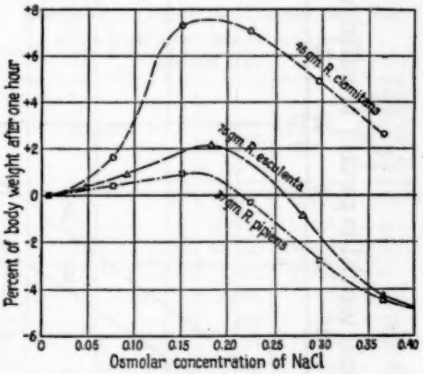

FIG. 2. INITIAL RATES OF NET WATER ACCUMULATION OR LOSS IN THREE SPECIES OF FROG

The average weight of one individual is indicated. The points for *Rana pipiens* and *Rana clamitans* are each the average of about 30 individuals. The points for *Rana esculenta* are for single individuals and are taken from Durig (28).

In frogs, osmotic pressure can be evaluated by placing individuals in various concentrations of medium, and noting the *initial* rates at which water is gained by the body (8, 13). Such a curve, in which each point represents the average result on about 30 individuals, is shown for *Rana pipiens* in figure 2. To give the rate of passage through the skin, this curve needs to be corrected for the accumulation of urine. The shape remains the same after correction, the rate of inward passage of

water being greatest at 0.16 osmolar concentration.

Osmotic pressure alone would cause an initial movement of water which is proportional to the gradient of concentration. It follows that the exchanges of water which begin when the frogs are put into the various concentrations of sodium chloride are caused in large part by forces

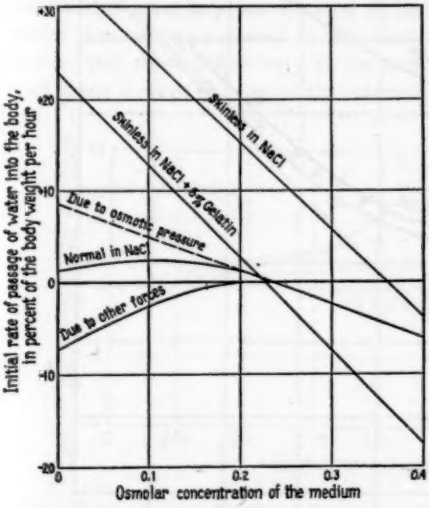

FIG. 3. COMPARISON OF THE RATES OF PASSAGE OF WATER INTO 37-GRAM *RANA PIPIENS* IN RELATION TO CONCENTRATION OF SODIUM CHLORIDE OF THE MEDIUM

Each curve is the smoothed average result for many frogs. The curve "normal in NaCl" represents the rate of passage through the skin of intact frogs having the cloacae bound so that urine could not escape.

other than osmotic pressure (13). Only in the higher concentrations, where the medium is more highly concentrated than the frogs' blood and lymph, do the rates of exchange of water give any appearance of being proportional to concentration.

The rôle of the skin may be distinguished by means of a parallel series of experiments in which the frogs have their skins removed, and are immediately placed in various concentrations of sodium chloride (15). These frogs are alive, maintain their circulations for at least an hour, but have their muscles, blood capillaries, and other tissues in contact with the medium. The initial rates of net water exchange are now proportional to concentration; the smoothed line for them is shown near the top of figure 3. The skinless frog appears to be an ideal osmometer.

But the difficulty may be noted that the skinless frog gains weight in concentrations up to 0.36 osmolar, which are much higher than the total concentration of the blood. The intact frog, on the other hand, neither gains nor loses water in about 0.23 osmolar, which is equal to the total concentrat on of the blood and lymph. It is possible that this peculiarity of the skinless frog is due to the absence of proteins from the medium. Another series of experiments with 6 per cent gelatin added to the medium of various concentrations of sodium chloride, shows this surmise to be correct. The concentration at which weight is neither gained nor lost, which might be considered an "isotonic" point, is now about 0.23 osmolar (figure 3). The difference between the solutions without and with gelatin is not due merely to the extra osmotic pressure of the gelatin, which at most amounts to 0.02 osmolar, but to the ready passage of sodium and chlorine into the tissues except when the skin is present.

Without the skin, all the other forces at work are evidently constant, so that only osmotic pressure exerts its effect. The slope of the line showing rates of migration of water into skinless frogs at various concentrations, is about 2.6 times greater than the slope of the line for normal frogs in the higher concentrations (figure 3). It is likely that this is due entirely to the smaller distance through which water has to diffuse in order to enter the circulation of the skinless indi-

viduals. Assuming this to be the case, we arrive at the probability that the slope of the line in higher concentrations for intact frogs represents the rate at which water would be entering or leaving the frog's body if there were no forces active except the one of osmotic pressure (15). This probability is rendered almost certain by the fact that after twenty-four hours in any *hypertonic* medium the osmotic pressure of the blood is very nearly equal to that of the medium. The data upon which this statement is based are shown in figure 4.

All other forces present can now be evaluated by merely taking the difference between the extrapolated value for the movement of water due to osmotic pressure, and the actual value found. These forces are significant in amount only in hypotonic concentrations of the medium. Further identification of the nature of

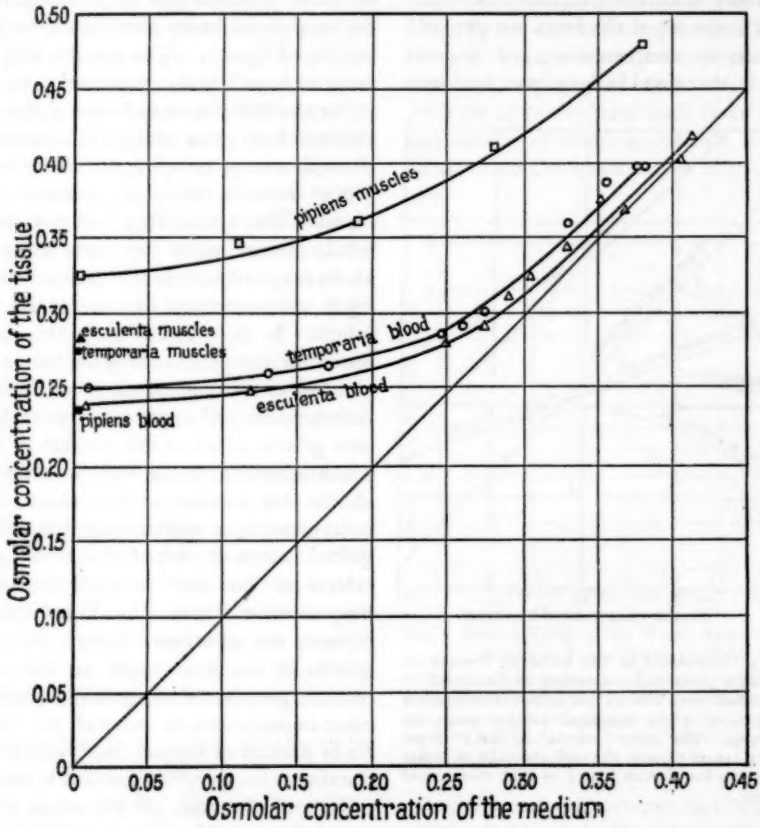

FIG. 4. EQUILIBRIUM OF OSMOTIC PRESSURE OF DEFIBRINATED BLOOD, OR MUSCLES, OF THREE SPECIES OF FROG IN MEDIUM OF VARIOUS CONCENTRATIONS OF SODIUM CHLORIDE

The data are for: Esculenta blood (22, 23); Temporaria blood (31); Pipiens blood (46); Esculenta muscles (34); Temporaria muscles (17); Pipiens muscles (13).

these forces, which reside wholly in the skin, cannot at present be made, though various possibilities have been tested through correlative studies of oxygen consumption (14), electrical potential, and temperature coefficient in relation to solutions.

The skin of a frog is a boundary through which water is exchanged much as it is exchanged through any other membrane. But in this boundary are situated forces which are not encountered in all membranes; and these forces vary in amount with many sorts of changes in the internal and external environments of the animal. The skin is not a mere "sieve," or "semipermeable," or possessed of "one-way permeability;" instead it is the seat of certain forces which modify the exchanges of water in compliance with conditions. It is possible that the permeabilities of all kinds of membranes can be evaluated by similar procedures in terms of the forces present.

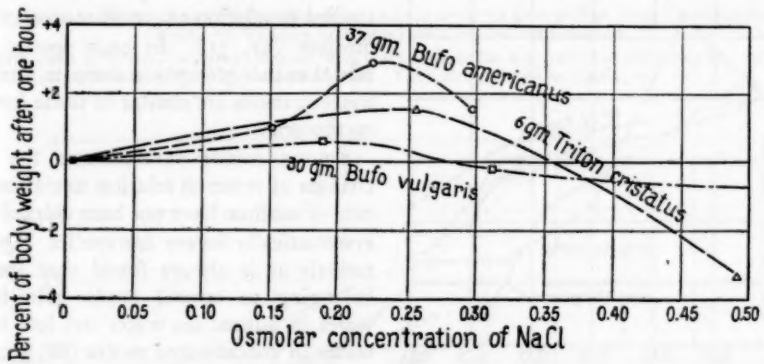

FIG. 5. INITIAL RATES OF NET WATER EXCHANGE IN ONE SPECIES OF SALAMANDER AND TWO SPECIES OF TOAD
The points for *Triton cristatus* and *Bufo vulgaris* were obtained on single individuals by Backman and Sundberg (17).

WATER BALANCE OF VARIOUS AQUATIC ORGANISMS

It is found that the intensities of the forces which reside in the skin of *Rana pipiens* vary with the time that the individual has remained in a given medium (13). For this reason if for no other it is not surprising to find that different species exhibit diverse responses upon being transferred to new concentrations of medium.

Four species of *Rana* have been tested in relation to concentration. Among these four the curves for rates of water exchange are roughly similar in shape; but very significant differences in magnitude are present. Three of these four are shown in figure 2; the curve for *Rana temporaria* (17) or *Rana fusca* (68) is nearly identical with the curve for *Rana esculenta*.

Only three other species of amphibia have been studied in relation to concentration of the medium. As shown in figure 5, *Triton*, a urodele, qualitatively resembles *Rana pipiens*, but the concentration at which its weight remains unchanged is much higher. The difference between the two species of toads, *Bufo*, which have been studied, is striking. It is certain that toads are not terrestrial organisms from the point of view of their water balance, for they exchange water through the skin equally rapidly with frogs, and they evaporate just as fast through the skin when taken out of water.

The larvae of amphibia differ from the

adults of the same species in their water balances when placed in diverse concentrations of the medium. *Rana pipiens* behaves very like the ideal osmometer at all ages from soon after hatching up to metamorphosis, as shown in figure 6. Upon the day at which it ceases to use its gills for respiration it changes its water exchanges to those characteristic of the adult (11).

Tadpoles of other species of *Rana* may differ significantly, as is the case for *Rana temporaria* (figure 6). Larvae of *Amblystoma*

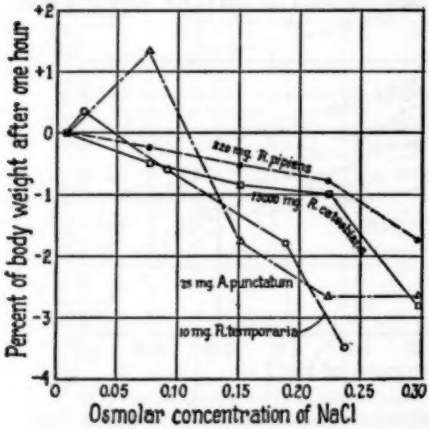

FIG. 6. INITIAL RATES OF NET WATER EXCHANGE IN FOUR SPECIES OF LARVAL AMPHIBIA (10)

Within a species the rates are less for larger individuals, but the shape of the curve is unchanged. The points for *Rana temporaria* are taken from Roaf (58).

punctatum show still another titration curve (10).

Among freshwater fishes the only curves which have been worked out do not resemble the curve in frogs (66, 45, 38). An illustration is *Gasterosteus* shown in figure 7. It is likely that still other types of curves will appear among fishes when more species have been studied in this particular manner.

Few invertebrate species have received study of the sort required. Planarians and earthworms may be judged from their initial exchanges in solutions of sodium chloride to be freshwater organisms of the type resembling the ideal osmometer (5, 9). But it is also true that in course of time they, and many other of the species mentioned above, acquire or exert forces which overcome the one of osmotic pressure. It is also obvious that another force must be ever present when these animals are in the initial or usual state, for their internal media are always more concentrated than the fresh water in which they ive.

Three unicellular species have been studied in relation to concentration of the medium (27, 33). In each species the initial exchanges upon transfer to various concentrations are similar to those for the earthworm.

Among marine invertebrates the exchanges of water in relation to concentration of medium have not been worked out systematically for any one species. Qualitatively it is always found that species belonging to several phyla gain body water in diluted sea water and lose body water in concentrated media (61, 57, 25, 26). Marine fishes appear also to obey this rule so far as the incomplete data go (66, 62).

Considerable information about the balance of water between organisms and environments is furnished by studies of the osmotic pressures of body fluids as modified by the concentration of the medium. Such data are available upon many marine species, for among them are a large number which can live comfortably in a wide range of concentrations, even in fresh water. Among freshwater species the range of species and of concentrations which can be studied is more limited.

When this correlation of body fluids and media is made, sufficient time for complete equilibrium having been allowed in

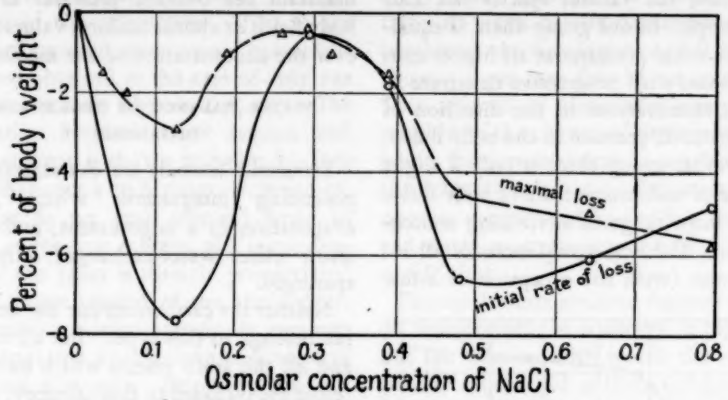

FIG. 7. LOSSES OF WATER BY THE FISH GASTEROSTEUS LEIURUS OF ABOUT 2-GRAM SIZE, UPON TRANSFER FROM TAP WATER TO VARIOUS CONCENTRATIONS OF SODIUM CHLORIDE

Initial rates are plotted as change of body weight per hour in the first half hour after transfer. Maximal losses are average values selected by the investigator. The data are from Gueylard (38).

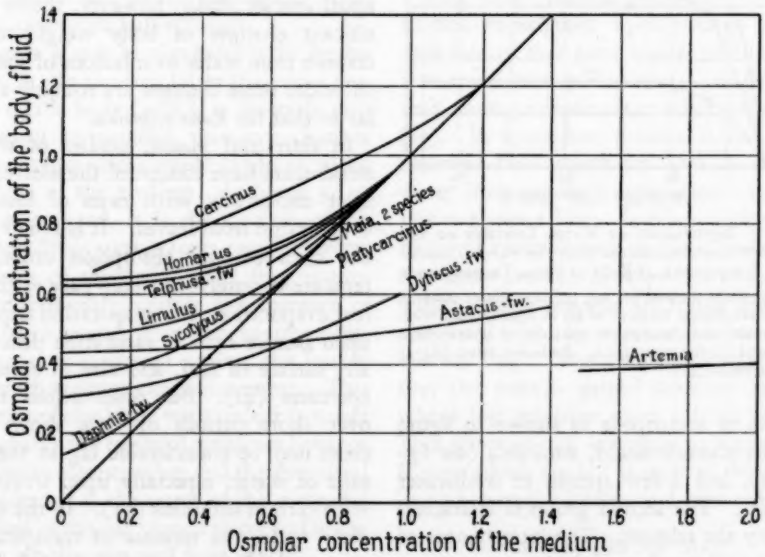

FIG. 8. EQUILIBRIUM OF OSMOTIC PRESSURE OF BODY FLUID OF VARIOUS SPECIES OF ARTHROPODS IN MEDIUM OF VARIOUS CONCENTRATIONS

The data are for *Carcinus* and *Maia* (35, 29), *Telphusa* (30), *Platycarcinus* and *Astacus* (29), *Homarus*, *Limulus* and *Sycotypus* (37), *Dytiscus* (16), *Daphnia* (36), *Artemia* (49). The four species marked "fw." normally live in fresh water.

every test, the various species fall into two groups. In one group there is equality of osmotic pressures at all higher concentrations, with progressive departure at lower concentrations in the direction of excess osmotic pressure in the body fluids. In the other group there is only a slight increase of osmotic pressure of body fluids over a large range of environing concentrations. The first group includes all invertebrates (with the exception of a few

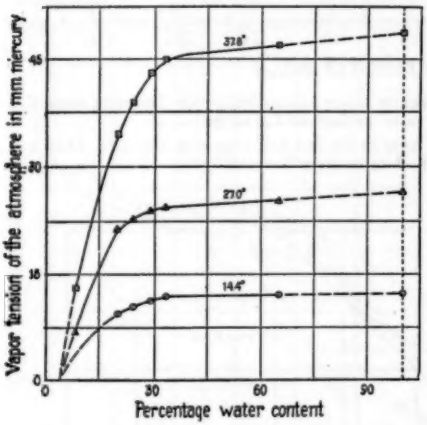

FIG. 9. EQUILIBRIUM OF WATER CONTENT OF THE MOSS HYPNUM TRIQUETRUM WITH THE VAPOR TENSION OF THE ATMOSPHERE, AT EACH OF THREE TEMPERATURES

The points plotted for 100 per cent water content are the saturation tensions of air in equilibrium with pure water, and these water contents of course were not found in the organisms. Redrawn from Mayer and Plantefol (48).

species of arthropods as shown in figure 8), the elasmobranchs, amphibia (see figure 4), and a few species of freshwater teleosts. The second group is characterised by the teleosts. The interchanges of fluid by all members of the first group must be qualitatively like those that have been described for the frog. The second group may include two kinds, those which do not interchange, as mammals, birds, reptiles, and some insects; and those which interchange substances freely, and yet maintain the osmotic pressures of their body fluids at almost uniform values whatever the concentration of the medium.

WATER BALANCE OF TERRESTRIAL ORGANISMS

Terrestrial animals are characterised by possessing integuments which retard evaporation to a huge extent, and which allow other water-exchanges only very sparingly.

Neither the earthworm nor any amphibian belongs to this type. But all reptiles and all the adult insects which have been tested are included in this category. *Phrynosoma*, *Alligator*, and *Chrysemys* when put in solutions of various concentrations exhibit no more exchange of water than when kept in water or put in air. The small species *Anolis*, however, shows significant changes of body weight upon transfer from water to solutions of sodium chloride; these changes are roughly similar to that for *Rana esculenta*.

In terrestrial plants, studies of water metabolism have concerned themselves almost exclusively with rates of loss by evaporation from leaves. It is found that the rates of root absorption ordinarily regulate themselves to keep pace with the leaf evaporation; the evaporation depends upon factors such as saturation deficit of air, surface of leaf, and size of stomatal apertures (41). But observations made over short periods of time show that there may be considerable lag in the uptake of water, especially upon treatment with certain solutions (53). In the usual daily cycle, the increase of transpiration at noon leads to a negative water balance throughout the afternoon. During the night water is then taken up to saturate all the tissues of the plant (41).

In any terrestrial organism the relative water content of the body is of course to be correlated not with the osmotic pres-

sure of a liquid medium but with the vapor tension of the atmosphere. This is the ideal information about water balance; it has been obtained in the case of only one species, the moss *Hypnum* (48, 54). The correlation between vapor tension and water content is shown in figure 9. It is plain that over a wide range, from saturation (at 65 per cent content) down to about 35 per cent content, the water content of the plant is directly proportional to the vapor tension of the atmosphere. It is unknown what specifically prevents supersaturation, that is, infinite dilution of the plant substance. Below 35 per cent content the curve of vapor balance is suddenly different, as though a new force were concerned in holding this water. At very low water contents, below 15 per cent, the forces which successfully resist evaporation are still stronger.

Figure 9 may be compared with figures 4 and 8. In the two latter the vapor tension of the body (which is inversely proportional to osmotic pressure) changes little over a considerable range of vapor tensions of the medium. In figure 9, on the other hand, it is the medium which changes little over a large range of water contents of the body. The frog is protected primarily against the entrance of too much water from the environment; the moss is protected against the loss of too much water to the environment. This contrast of equilibria with respect to water is probably the essential difference between the freshwater organism and the terrestrial organism; it is the secret of the successful survival of the one in pure water and of the other wholly removed from water.

The vapor tension curve for the moss is such that when its water content is as high as 60 per cent, water will leave a strong salt solution such as 2M potassium nitrate to go into the moss. If moss which has been equilibrated with pure water is put into various strengths of potassium nitrate; its water content diminishes in proportion to the concentration, but only by about 3.5 per cent for 2M concentration. This amount of diminution corresponds roughly to the change of vapor tension in going from pure water to a solution of this strength in figure 9. Its exact evaluation is complicated by the fact that salt and water penetrate into the moss almost equally fast.

The equilibrium attained depends largely on temperature, as is shown in figure 9. But the velocity with which the equilibrium is approached is little influenced by temperature. Moreover, the equilibrium and the velocity are the same for moss which has been boiled at 110° as for living moss (54).

This study should point the way to fruitful experiments upon insects. It is well known that some insects will lose no water, and even gain in relative water content, during complete starvation and thirst (19). In a number of cases it has been observed that insects are able to absorb water from a humid atmosphere (21, 20) just as *Hypnum* does. In place of measurements of vapor tension of the atmosphere, the water content of wheat in and upon which certain weevils are living may be correlated with the water content of these weevils (59). The result is curious in that the weevils gained moisture as the wheat lost moisture, over a large range. This serves to call attention to forces and factors which are not present in the equilibria between solutions and atmospheres.

WATER BALANCE OF MAN

Man may be taken as the type of terrestrial vertebrate. His content of water is surprisingly constant from day to day, when it is considered that he loses, each 24 hours, five per cent of the quantity present (table 2). Not having an environ-

ment of water, it is necessary to take into the alimentary tract an adequate or an excessive amount. The behavior which is aroused through the sense of thirst appears normally to take care of this need. The main difference from the freshwater animal is that *all* the water in contact with the absorbing surface is absorbed into the circulation, for normally no significant quantity of water passes along throughout the intestine.

Experiments are readily performed in trying to ingest water faster than it is eliminated as urine (1). Whereas ordinary catabolism furnishes about 40 cc. of urine per hour in an average man, rates of excretion up to 2000 cc. per hour have been reported, and nearly any individual can attain 1000 cc. per hour. It is physically impossible to ingest water at a much greater rate than this for many hours on end; so that absorption and elimination both go to completion without any measureable net retention or loss of body fluid.

By profuse sweating also it is possible to lose up to 2000 cc. of water per hour from the body (3). Could a man continue to ingest 2000 cc. per hour, he could either by diuresis or by sweating completely replace the body's complement of water in about 26 hours. This extreme treatment leaves the body with almost exactly the volume and concentration that it originally had.

On the other hand, it is possible to deny water to the body. Ten per cent or more of the body weight may be lost without unusual symptoms appearing (1). This loss may be completely replaced by drinking water alone, and if drinking continues after repletion, then diuresis occurs to carry away the excess. The precision

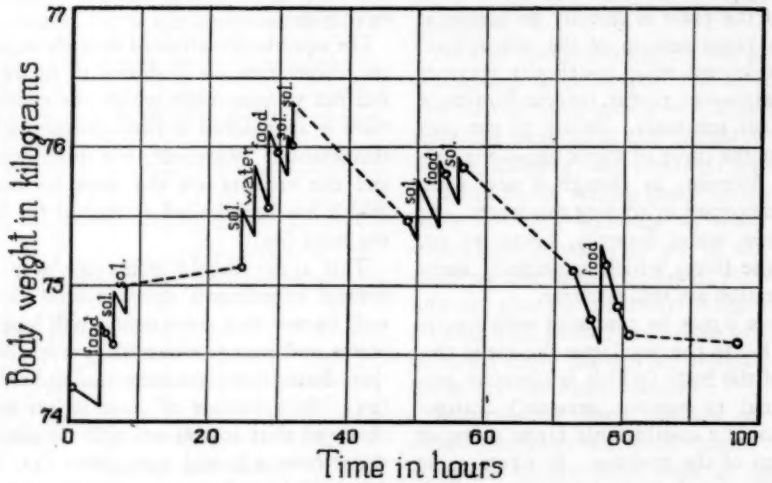

FIG. 10. CHANGES OF WATER BALANCE, INDICATED BY THE BODY WEIGHT, OF A MAN INGESTING A LARGE QUANTITY OF SALT IN ADDITION TO THE REGULAR DIET

The attempt was made to ingest salt water faster than it could be eliminated. Within 50 hours, 52 grams of NaCl and 25 grams of $NaHCO_3$ were taken, with enough water to form a solution of about three times the osmotic pressure of blood serum. Considerable additional water could be ingested at any time without producing a rapid diuresis, as is shown at the twenty-seventh hour. After the second day no lasting gain of weight was obtained, and the rate of elimination of water was increased.

with which the original body weight is again reached in such an experiment is astonishing.

How does the body know when its proper water content is present, so that water in excess is not retained, and water short of saturation is tenaciously held? Saturation evidently is related to concentration; and it is legitimate to refer the water to the amounts of salts and the salts to the amounts of certain organic substances such as certain proteins, leaving to one side the problem of how the amount of these in turn is regulated in body or cell. Even the blood regulates its concentration and its volume with such an accuracy that dilution can scarcely be detected during a large diuresis (55). The body responds by distinctly increased water excretion to an ingestion of as little as 100 cc. (2), which gives a decrease of concentration of less than 0.2 per cent if this water becomes distributed equally throughout the body.

Though the body does not retain pure water that is administered in excess of saturation, certain solutions will be retained for longer or shorter periods. An experiment of this sort is shown in figure 10. The mixture of NaCl and $NaHCO_3$ which is ingested along with less than enough water to make it approximately isotonic with blood, does not lead to a sudden faster elimination of water through a diuresis such as follows water-drinking. There is instead a prolonged slight increase in the rate of urinary water excretion, so that body weight is lost more rapidly than on an ordinary diet. Similar experiments have often been reported (47, 39, 50, 1, 18).

A large number of different solutions may be compared in order to see which salts hold water in the body for the longest time (2, 7). Such experiments are shown in table 3. The salts which do *not* give rise to prompt excretion of the water are of two kinds: first, those like sodium chloride, which seem to simulate closely the composition of blood plasma, and second, those like calcium chloride, which are not absorbed to any large extent. Sodium bromide imitates sodium chloride; while sodium iodide and potassium chloride do not. Fluid of a composition similar to blood plasma seems to deceive the regulatory tissues into retaining it over a number of hours or days. But within two or three days the body weight regulates to normal.

TABLE 3

Rates of elimination of water by a man after drinking, in each experiment, 1000 cc. containing a salt in a concentration approximately equal to that of the blood plasma

SOLUTE	CONCENTRATION IN PER CENT	CONCENTRATION OSMOLAR	TOTAL CC. EXCESSIVE WATER EXCRETED WITHIN 6 HOURS	MAXIMAL RATE OF WATER EXCRETION, IN CC. PER HOUR
—	0	0	970	790
NaCl	1.0	0.31	190	111
NaBr	1.7	0.30	50	75
"Locke"	0.9	0.28	80	49
NaCl+$NaHCO_3$	0.75+0.36	0.31	280	114
$NaHCO_3$	1.45	0.31	490	416
NaI	2.6	0.31	670	650
KCl	1.3	0.31	750	460
KCl+NaCl	0.43+0.67	0.31	690	810
KCl+$CaCl_2$	0.65+0.85	0.31	340	330
Urea	1.9	0.32	560	673
$CaCl_2$	1.7	0.37	70	93
Na_2SO_4	1.8	0.32	50	71
Na_2HPO_4	1.7	0.30	60	75

It must be obvious that water taken simultaneously with food is also usually

retained. During the periods between meals catabolism is constantly reducing the organic constituents of the body to carbon dioxide, urea, and other substances. As these are formed and excreted, water is also formed and liberated from the tissues, and it likewise is excreted. At the absorption of food, anabolism builds water into the tissues along with glycogen, amino acids, and the like. Since all food which is absorbed is retained by the body without limit, it is natural to ask whether water is retained in proportion to the quantity of food. Experiments show that the proportion between water and food is not a direct one, even for a single food substance. The correlation of the amount of water with foods of different characters is still more complex. And the rôle of salts in conjunction with these foods is at present not at all understood.

Water may be made to stay in the body over shorter or longer periods by the administration of various specific substances. Pituitrin is one of these that has received particular study (55, 7). Its constant presence in the normal body suggests that it plays a major rôle in the normal everyday water-balance. It is supposed by some investigators that the rate of activity of the posterior lobe of the pituitary gland may make the difference between obesity and slenderness. Other endocrine substances affect water-balance either directly, or through the pituitary, or through the liver, or through the circulation (51, 52, 44). Numerous drugs influence water-balance over relatively short periods of time.

DISTRIBUTION OF WATER AMONG TISSUES

The water-balance of the body must be considered also in relation to the individual tissues. Measurements of the water content in plethora and in desiccation have been made in various mammals (32, 64, 43) and in frogs (67, 40), under conditions of plethora and thirst. Undoubtedly the shifts of water among tissues must be analysed in the same way as the shift between organism and environment, before the regulation of water content will be understood.

It must be pointed out, as is indicated in figure 4, that various tissues normally differ from one another in their osmotic pressures or vapor tensions, yet are in perfect fluid equilibrium.

A few kinds of freshly isolated animal and plant tissues have been studied with respect to the exchanges which occur when they are put into media of various concentrations. Thus, slices of potato or of carrot give initial rates of water exchange which are nearly proportional to concentration (65). There is no exchange of fluid in an isosmotic concentration. Mammalian connective tissues (60), frog muscles (42), and skin (15), have been studied in a similar way, with like initial results.

WHAT HOLDS AND REJECTS WATER?

There is no doubt that the water present in tissues is held there in a variety of ways. Some of it depends upon osmotic forces, some upon colloidal forces, some upon chemical forces. It is possible to think of the tissues or cells as sponges, holding water up to a certain saturation level. But it seems less promising to consider each tissue as a block of protein-gel containing various partially diffusible compounds than to consider each tissue as a heterogeneity of membranes. Just as the rate of passage through the skin of the frog is studied in relation to the various forces operating, so the exchange through each tissue structure and cell structure may eventually be measured.

The ultimate problem of the regulation of water balance must recognize not merely the concentrations or proportions of each

constituent and force. For, most organisms, though materials be supplied in the proper relative amounts, do not at all times incorporate them. Rather, regulators of water balance and of any other balance are able somehow, directly or indirectly, to ascertain and control the *absolute* quantities of substance. Were content merely a matter of proportions, all constituents would be lost at the same relative rates throughout total starvation.

Problems of growth, size, and proportions are inseparable from the problem of water balance. Essentially water balance is independent of biochemical and nutritional factors, for such factors may easily be supplied; there then manifests itself an organism which goes on regulating its own constitution.

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SEX, SPECIES, AND RACE DISCRIMINATION BY MANOILOV'S METHODS

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INTRODUCTION

EVIDENCE purporting to discriminate the sexes and to afford a classification of individual groups of organisms on the basis of physiological and chemical differences has been the subject of many speculative discussions. Speculation on this question is, however, premature, for despite the rather impressive amount of statistical and experimental data that has accumulated since such work was begun, we have as yet no sure way of ascertaining whether the differences involved are of a qualitative, quantitative, or environmental nature. Manoilov attempted to ascertain the nature of the differences involved and presented certain chemical methods the application of which, he believed, could produce characteristic reactions by which male and female blood and tissue extracts as well as the blood and tissue extracts of groups of organisms could be easily differentiated. He attributed these reactions to qualitative differences in the blood.

Before taking up Manoilov's work, it will be advantageous to consider briefly the various views concerning sex differentiation and views concerning differences between groups of organisms. Two theories in regard to the factors involved in these phenomena of sex are prevalent. The first is that difference of sex is predetermined by the presence or absence of a particular chromosome. This contention is supported by genetic evidence. Second, physiological studies suggest that sex is differentiated either by differences in metabolic level (quantitative differences) or by the elaboration of specific secretions called hormones (qualitative differences). Moore ('25) suggests that these two views are not contradictory but are merely different manifestations of the same physiological condition. Riddle ('14 a and b, '15, '16, '20, '23, and '24), on the basis of his investigations, is of the opinion that sex is not "fixed" but "plastic," and is dependent upon the environmental conditions.

The fact that qualitative chemical differences between groups of organisms do exist is conclusively proved by the work on sero-diagnosis (Mez and Ziegenspek, '26) as well as by that on the chemical constituents of respiratory pigments. From serological studies we learn that organisms may be divided into groups on the basis of agglutinating reactions of the blood. It has been assumed that these different reactions are produced by specific substances the chemical nature of which is unknown. Landsteiner's ('00) studies on human blood have shown that there are four blood groups in the species and that they have a wide geographic distribution.

Again we find that organisms fall naturally into groups on the basis of differences in the crystal structure of their respiratory pigments. Reichert and Brown's ('09) extensive studies on the crystallography of the haemoglobins indicate that these substances exist in a great number of modifications "each modifica-

tion being characteristic of the form of protoplasm, the organ, the individual, the sex, the species, and the genus." On the basis of these modifications, as evidenced by differences in crystal structure, the groups (genera, species, sexes, etc.) may be easily differentiated. These are probably diversities in chemical constitution based on stereo-isomerism, but the work of Willstätter and Stoll ('13) on chlorophylls indicates that at least in this group specific differences in chemical composition also exist. Some indication of similar differences in the haemoglobin may be seen in Helff's ('27) work on the different rates of oxygen consumption in the larvae of various species of *Amblystoma*. It is very probable that this differential rate of oxidation is correlated with specific chemical characteristics of the respiratory pigment.

DISCRIMINATION OF SEXES

Manoilov had long been interested from the standpoint of forensic medicine in the problem of the determination of sex by means of blood reactions, but it was not until 1920 that he first succeeded in discriminating male and female blood, even in a small percentage of the cases. In the two years which followed, he devoted his time exclusively to this problem; working by the method of trial and error, changing and modifying his technique until he had arrived at a method which gave a much higher percentage of correct results than any other. From the thirty-six modifications used, he picks three which he says give the best results. Of these the "Third Modification" is the one which is now used almost exclusively by Manoilov as well as by other investigators. The method is as follows (Manoilov, '23 a):

To 3 cc. of unwarmed erythrocyte emulsion the following reagents are added in the order given:

1. 1 per cent aqu. solution of papayotin Merck (filtered) 10 drops.
2. 1 per cent alcoholic solution of dahlia (Grübler) or methyl violet (filtered) 3 drops.
3. 1 per cent aqu. solution potassium permanganate 10 drops.
4. 1 per cent hydrochloric acid (40 cc. conc. hydrochloric acid plus 60 cc. water) 3 drops.
5. 2 per cent thiosinamine solution 5 drops.

Manoilov placed special emphasis upon the addition of the reagents in the proper order. Moreover, the solution must be allowed to stand 1–2 minutes after the addition of the first reagent, it must be stirred but not shaken after the addition of each of the first three reagents and be shaken after the addition of the fourth. If all these precautions have been properly observed a solution containing male blood should be colorless, while one containing female blood should show a violet tint.

In preparing the blood emulsion Manoilov added 4–5 volumes of physiological salt solution to 1 volume of blood and stirred this until a semi-transparent (*halbdurchsichtige*) emulsion was obtained. Emulsions of greater or less concentration may also be used, but it is necessary in such cases to alter in a corresponding way the number of drops of the reagents added. That is, the dye and the other reagents must be standardized with respect to the emulsion used. Thus, if a certain emulsion shows no color either with male or female blood, it is necessary to decrease the number of drops of hydrochloric acid added until the male blood alone becomes colorless while the female remains partly colored. Decreasing the amount of the fifth reagent will also accomplish this purpose. If, on the other hand, the emulsions from both sexes give a color, then the amount of dye added must be decreased until a differentiation is obtained.

Using this method Manoilov declares that he obtained positive results in 86–96 per cent of the 530 cases investigated. In many instances, however, he obtained

"reversals," male emulsions gave female reactions and vice versa. In these the investigator introduced a "correction." If a male emulsion gave a female reaction he added 10–20 drops more of potassium permanganate, 2 drops of hydrochloric acid and sometimes 3 drops of thiosinamine. After these additions and after shaking, the fluid usually lost its color. In case of female emulsions which gave the male reaction, more of the third reagent was added; whereupon a darker color was obtained. As a control the same number of drops of the reagent were added to a typically colorless male emulsion and shaken; the emulsion remained colorless, or became slightly darker. By this means Manoilov was able to greatly increase the percentage of correct results obtained. However, if we grant the validity of such a procedure, which is at best questionable, it is evident that it could not be used in cases in which the sex is unknown, for in such instances we could have no means of recognising a "reversal" as such. For this reason the "correction" procedure has very little practical value.

This first work related entirely to the determination of the sexes in animals, but it was not long before Manoilov ('23 b) extended his work to a consideration of the same problem in plants. In view of the close chemical relationship which has been demonstrated by Nencki, Marchlewski and Zaleski (see Reichert and Brown, '09) to exist between chlorophyll and haemoglobin, it seemed not at all improbable that they would show similar chemical relations in this case. Manoilov investigated dioecious plants by extracting the green leaves with 60 per cent alcohol for 1–2 hours at room temperature. The reagents of the third modification were added to 3 cc. of such extracts and in all cases correct results were obtained.

The publication of these results attracted wide interest and the question of the chemistry of the reaction involved and the rôle of the various reagents quickly came to the fore. Manoilov himself said little on this subject in his first two papers ('23 a and b), but in his third article ('24) he sets forth his ideas on these points and gives the results of some experiments undertaken in order to determine which reagents play the most important part in the reaction. For this purpose he made a number of tests with male and female blood, adding the reagents in the proper order but omitting one or the other of them. In this way he found that the omission of any of the five reagents prevented the reactions from occurring and no discrimination was obtained. (These results, it may be remarked, do not accord with those of later investigations.) From the consideration of experiments of this nature, Manoilov recognised that an oxidation process occurs in the reaction and he assumed that the papayotin and thiosinamine have the most important rôle to play; the dahlia, potassium permanganate, and hydrochloric acid merely exerting an oxidation effect. ("Aus der Tabelle ist ersichtlich, dass bei der Reaktion ohne Zweifel ein Oxydationsprozess vor sich geht. Ausserdem ist deutlich zu erkennen, dass Papayotinum und Thiosinamin die wichtigste Rolle spielen. Die Reagense N.N. II, III und IV beteiligen sich und üben bei der Reaktion eine Oxydationswirkung aus.") The basis of the reaction, he thinks, rests upon a qualitative difference between the bloods of the two sexes, a difference which is due to the presence of male and female sex hormones which produce certain slight changes in the blood that are discoverable by this procedure.

DISCRIMINATION OF RACES

Manoilov in 1925 presented a series of empirical methods by means of which

certain nationalities could be discriminated chemically. In view of the fact that serological studies have shown that there are specific chemical differences between groups of individuals, a specific substance, analogous to the sex hormones differentiating the sexes, was supposed by the author to be responsible for these differences. To quote, "Auf Grund der Tatsache, dass die Geschlechtsbestimmungsreaktion durch Blut die Unterscheidung der Geschlechter voneinander ermöglicht, war es für mich klar, dass bei Vorhandensein von Hormonen für die beiden Geschlechter- ganz analog auch im Körper spezifische Rassensubstanzen ausgescheiden werden müssen, die den verschiedenen Rassen einen ganz spezifischen Charakter verleihen." He thought that these methods gave an index to the chemical properties of the unknown specific substances characterizing races and nationalities. These methods were of the same general nature as those that were used for the discrimination of sexes. According to this author, however, the question of sex plays no important part as far as these methods are concerned.

The following is the method used to discriminate Russian from Jewish blood (Manoilov, '27):

> For these reactions the following reagents are needed:
>
> 1st reagent—1 per cent solution of methyl blue (Grübler).
>
> 2nd reagent—1 per cent alcoholic solution of cresyl violet (Grübler).
>
> 3rd reagent—½ of 1 per cent solution of silver nitrate.
>
> 4th reagent—40 per cent solution of hydrochloric acid.
>
> 5th reagent—1 per cent aqueous solution of potassium permanganate (freshly prepared).
>
> The reaction proceeds in the following way: Add to 3 cc. of the unheated emulsion of red blood corpuscles (3–5 per cent) or directly to the coagulum of blood three to four times that much (in volume) sodium chloride (physiological solution) and mix with a glass stick so as to obtain a rather thick emulsion. Then add 1 drop of the first reagent, shake; 5 drops of the second reagent, shake again; 3 drops of third reagent, shake; 1 drop of fourth reagent, and lastly 3 to 5 drops of the fifth reagent. The result will be correct if the fluid with Jewish blood proves to be paler than that with Russian blood. In the Jewish blood the cresyl-violet must disappear entirely or almost so, and the blue or blue-green shade alone remain; in Russian blood the cresyl-violet will partly remain insoluble, and we see generally a blue-red coloring.

The blood was obtained only from individuals whose hereditary history had been investigated, i.e., those that had had three or four paternal as well as maternal ancestors of the same nationality. It was upon these results that the conclusions were based. When this method was applied to Russian and Jewish blood the resulting fluid was a paler shade of green with the Jewish blood than with the Russian blood. Manoilov explains this phenomenon by saying that the "cresyl-violet dissolves in the Jewish blood, whereas in the Russian blood it dissolves only partly but never completely." To get good color difference the author suggests modifications of the amount of reagents added.

No clear cut notion can be obtained from this work concerning the nature of the chemical reactions involved or the functions of the various reagents used. The impression is conveyed that by incorporation of the various ingredients prescribed, in the manner of a cook book recipe, in which so much of this and so much of that is added, some kind of result is obtained, sometimes good and sometimes bad. Cresyl-violet disappeared because it became oxidised. Oxidation, it was supposed, took place only in the presence of finely divided silver chloride, which is formed as the result of the reaction, and substances (no mention is made of their nature) can by this means be

eliminated from the field of action by adsorption. That chemical discrimination between nationalities rests upon varying degrees of oxidation seems to be the predominating idea.

From these results it was concluded that oxidising processes go on more readily in Jewish blood than in Russian. This method has been found accurate according to Manoilov in 91.7 per cent of the cases tested, that is, when pure Russian and pure Jewish blood are tested. The blood of other nationalities: Chinese, German, Esthonian, Finnish, Polish and Armenian, was likewise studied, but owing to the lack of material no definite conclusions were drawn concerning the results. However, these other nationalities, Manoilov, says, could be readily discriminated from one another by this means.

The blood of children born as the result of mixed marriages was also studied. The blood of the offspring from a pure Russian father and a pure Jewish mother gives a resulting color that is stronger than the color obtained from the Russian. Again if the father was pure Russian and the mother (pure?) German, the result differs from the purely Russian. From this evidence it was concluded that there were indications of the influence of one nationality upon another. In one case the paternity of a disputed child was determined by the use of this method.

DISCRIMINATION OF SPECIES

Since Manoilov considered that he had succeeded in discriminating nationalities, he next turned his attention to the chemical differentiation of species (Manoilov, '27 b). Working upon the basis that the physico-chemical condition of the blood is the key to the solution of the problem of the origin of species, a series of chemical procedures was presented. These were modifications of the one used for the chemical differentiation of nationalities. With these modifications a correct answer in 100 per cent of the cases investigated was obtained, according to Manoilov, when cattle and horse sera were used. Sera of pigs, dogs and man were likewise used. However, no detailed tables of results were published. It was hoped by this investigator that by using blood of cattle and horses, where large quantities of blood are available, the reaction between the serum and the reagents in the procedures in question might be understood. These methods were derived likewise upon an empirical basis. As in the method for the discrimination of races, sex appears to have no importance.

The following method was used and recommended by Manoilov. It is called the "First Modification" and is quoted from his paper:

1. Reagent Brenzkatechin, 2 per cent of aqueous solution.

2. Reagent Methyl blue, 0.1 to 0.2 per cent aqueous solution.

3. Reagent China blue, 0.5 per cent aqueous solution.

The reaction must be performed as follows: Take test tubes as sterile as possible or at least irreproachably clean, into which pour 3 to 5 cc. of unheated serum; then add five drops of the first reactive-Brenzkatechin; shake; add next one drop of second reagent, methyl blue, shake thoroughly once more; finally add 1, 2, or 3 drops of the third reagent, china blue, and shake violently.

After the addition of the above named reagents, the serum of horses turns green to greenish, while the serum of cattle turns from blue to dark blue. To facilitate the color difference normal saline solution is added. There were three other modifications, using different dyes which gave similar results. China blue was, however, used in all of the modifications.

At first fresh serum was used; later it was found that the same result was ob-

tainable with old serum, the only difference being that of intensity of color. From this it was concluded that the reaction was chemical rather than biological. Since a correct result is obtained with coagulated serum the conclusion was reached that the unknown substance is not an enzyme but is an albumin complex; there being a characteristic one for each species.

The functions of the respective reagents and the nature of the reactions involved in these procedures is likewise not understood. A biacid salt of methyl blue is postulated. Brenzkatechin (O-dioxybenzol) was chosen because it was very susceptible to oxidation. China blue is added as an intensifier: it has no effect upon the resulting color of the horse serum but intensifies the shade of color when cattle serum is used.

A REVIEW OF THE WORK OF OTHER INVESTIGATORS

The results which have been reviewed have attracted the attention of many investigators, for it was seen that if the method could do all that was claimed for it, it would be of the utmost importance in many different fields. A number of workers set out to repeat Manoilov's work and to determine the limits of applicability of his method by means of experimentation upon extracts from various plants and animals. Much of this work gave confirmatory results but from the very start a large number of failures was recorded; some could no doubt be attributed to inexperience, but others seemed to justify the conclusion that the procedure was not applicable to these cases.

Calisto ('26) experimented with guinea pigs and found that this method of sex discrimination failed entirely; males gave the reaction of females as often as they did that for males and vice versa. He therefore concluded that the reaction could not be applied for the differentiation of sexes in guinea pigs.

Banta and Satina ('25), on the other hand, obtained positive results with *Cladocera* and found that parthenogenetic females gave a result intermediate between that of normal males and normal females.

Satina and Blakeslee ('26a) followed up this work by a study of plus and minus mucors in which positive results were obtained in about 95 per cent of the cases. The same investigators ('26 b) applied Manoilov's method to tissue extracts of the green leaves of dioecious plants and again obtained positive results but found differences between old and young leaves and between tissues extracted for varying lengths of time. In view of the fact that in Satina and Blakeslee's experiments with plus and minus mucors a definite biochemical difference was established on the basis of the catalase content, the foregoing results seemed to indicate that the Manoilov reaction was of a quantitative rather than of a qualitative nature, and it therefore seemed imperative that an exact study be made of the chemical nature of the method.

The first thorough analytical investigation of the mechanism of the reaction was made by Alsterberg and Håkansson ('26) and was confirmed in the main by the work of Galwialo, Wladimirow, Winogradow, and Oppel ('26), carried out independently at about the same time. Alsterberg and Håkansson tested a great number of different sorts of extracts of both animal and vegetable origin according to Manoilov's method and found first of all, that the test was noticeably affected by the dilution of the extract; as the concentration of the extract was decreased the final color of the test became weaker. This would indicate that there is some

substance in the blood which, if present in large amount, causes a darkened color of the test; if it is present only in small amounts a lighter color is obtained. Since the reaction is undoubtedly an oxidation-reduction process they concluded that the substance involved is probably a reducing agent. The result of the test would then depend upon whether the oxidising agent or the reducing agent is present in excess. In order to demonstrate that this view is correct, the investigators made some experiments, using instead of blood or plant extracts, a number of different reducing agents such as sodium bisulphite, sodium nitrite, oxalic acid, etc., adding the reagents of the Manoilov procedure to solutions of these substances of varying concentrations. In this way a whole scale of different colors was obtained according to the concentration of reducing agent used. This point is illustrated in the following table taken from their paper:

Natriumbisulfit, 2 proz. wässerige Lösung; zur Probe wurde (nebst 3 ccm Aqua dest.) die unten angegebene Anzahl Tropfen hinzugesetzt. Im übrigen erfolgte die Ausführung der Probe nach *Manoiloffs* Vorschrift.

BISULFIT ANZAHL TROPFEN	FARBE DER PROBE
1	farblos
2	farblos
3	farblos
4	rosa
5	ziemlich stark violett
6	sehr stark violett
7	noch stärker violett

This would indicate that the Manoilov reaction depends not upon the presence of any specific substance like a sex hormone, but rather upon the relative amount of organic substance present in the extract.

These investigators then proceeded to study the various reagents used in the method with respect to their function and specificity. They found that the papayotin, far from playing the important rôle that Manoilov had assigned to it, could be replaced by a number of purely inorganic reducing reagents like sodium bisulphite and could even be omitted without seriously affecting the result. They concluded that it simply served the purpose of making up a sufficient quantity of reducing agents necessary for the test. As for the dye (dahlia) its main function is probably that of an indicator. The permanganate serves as an oxidising agent which, by its reaction with the dahlia, gives an indication of the amount of reducing substances present. If there is an excess of reducing substances, a part of the potassium permanganate will go to oxidising them; as a result there will not be sufficient oxidising agent to completely decolorise the dye; thus a colored solution (female reaction) will be obtained. On the other hand if there is an excess of oxidising substances present the dye will be completely oxidised and the male reaction (colorless) will result. This occurs most readily in an acid medium which is supplied by the hydrochloric acid. The fifth reagent, thiosinamine, is another reducing agent, which by its addition after the foregoing reaction has occurred, serves to reduce the excess of permanganate and the manganic oxide to colorless compounds so that the color of the solution is more easily seen. On the basis of these results the authors are led to conclude that the reaction depends upon a quantitative rather than a qualitative difference between the sexes. If this is so it indicates that, in general, reduction processes prevail in female organisms while oxidation processes prevail in male ones. Alsterberg and Håkansson, however, deny that the method has such universal applicability as has been claimed for it, pointing to the fact that failures and reversals are often obtained, for the

reason that the amount of reducing substances present in the same organ may change considerably even in a short time. In other words they believe that the greater or smaller amount of reducing substances present is not at all specific for the sexes and since this is really what the Manoilov test measures, its accuracy cannot be confirmed.

The work of Galwialo, Wladimirow, Winogradow and Oppel ('26) was undertaken with the same object as that of Alsterberg and Håkansson and was pursued along much the same lines. They carried out a number of experiments in order to determine the part which the various reagents have to play in the reactions involved and according to them the mechanism of the reaction is explained in the following way: The permanganate oxidises the dye in acid medium, whereupon the latter loses its color. In the presence of substances which are more easily oxidised than the dye, however, the amount of permanganate does not suffice and the fluid under investigation remains colored. The thiosinamine serves for reduction of the manganic oxide which is formed. Papayotin is not necessary for the reaction. These results accord perfectly with those of Alsterberg and Håkansson. Galwialo and his co-workers now proceeded to an investigation of the specificity of the reaction. They tested a number of substances with respect to the ease with which they could be oxidised by potassium permanganate and found that the proteins take the first rank in this regard, especially the tyrosine, tryptophane and pyrrol derivatives. All of these are able to reduce large amounts of permanganate, but the reactions with tyrosine and tryptophane become negligibly small under the conditions of the Manoilov reaction (acid medium). Therefore these authors are led to believe that the reaction depends mainly upon the relative amounts of pyrrol derivatives present in the substance under investigation. The test is not specific, however, for many other organic compounds are able to reduce potassium permanganate under the conditions of the Manoilov reaction, although to a much slighter degree. This work, the authors believe, throws some doubt upon the investigations of the workers who have assumed a specificity of the reaction but have not taken into account the protein content of the fluids investigated.

Schmidt and Perewosskaja ('26) came to much the same conclusion on quite different grounds. They found that female blood serum has a higher specific gravity than that of male serum, owing to the fact that female serum contains an average of 8.5 per cent more proteins than does the male. The Manoilov reaction then, according to their view, merely furnishes an indication of the relative amounts of protein present. It is obvious that, upon this hypothesis, the "reversals" so frequently obtained are readily explainable, for it would only be necessary that a male serum have an abnormally high protein content in order to give a reaction indicating that it belonged to a female or that a female serum have an abnormally low protein content to give a male reaction.

This explanation is borne out by the work of some other investigators. Riddle and Reinhart ('27), working with pigeons and ring doves, found frequent reversals when examining blood samples from reproducing individuals; thus indicating that at that time the blood of the male contained more organic substances and the blood of the female contained less organic substances than normally. Riddle, in earlier studies, had shown that during the reproductive period there is an in-

crease in metabolism in the female and that it is probable that there is a corresponding decrease in the male. He therefore concludes, that the amount of organic substance in the blood varies with the metabolic rate and that the Manoilov reaction constitutes a test for metabolic level, which can be used as a sex test only at those periods when the metabolic rate is fairly constant. This hypothesis may explain the failure which Calisto encountered with guinea pigs, since he took no account of the oestrous period, state of sexual excitement or other factors which might have influenced the metabolic level of his individuals. Further experiments on these animals, with due consideration of the factors listed above, may go far toward explaining Calisto's results and substantiating Riddle's conclusions.

Falk and Lorberblatt ('27) experimented with the Manoilov method for the discrimination of the sexes and found that dahlia could be replaced by pararosaniline, the simplest representative of the triphenylmethane series of dyes, and that the papayotin could be replaced by proteins such as albumin. It must be noted, however, that such compounds must invariably be present. The thiosinamine may be replaced by various reducing substances such as sodium thiosulphate, ferrous sulphate, oxalic acid, hydrogen peroxide, so that its function must be to effect the reduction of the excess potassium permanganate. These investigators also found that a variation of the concentration of the original extracts does not influence the result if the concentration of the oxidising agent is suitably adjusted. This work was carried out with standard ovarian and orchic extracts and indicates that the reaction involved in the Manoilov method is essentially an oxidation reaction, although the exact part played by the ovarian and orchic substances and by the various materials from other sources in analogous tests is uncertain.

Poliakowa ('27), made use of the Manoilov method for race determination. The blood was obtained from ninety-nine individuals of mixed and pure marriages. In some instances blood was taken from father, mother and child. When the father and mother were of the same nationality it was called a "pure marriage" and when the father and mother were of diverse nationalities it was called a "mixed marriage." Before carrying on the work, Poliakowa first determined the resulting color of the reaction with the bloods of different nationalities. It was found that each nationality had a characteristic color. The following is a table giving the shade of color for the different nationalities (table 1):

Russian	reddish
Jews	blue-greenish
Esthonians	reddish-brownish
Letts	reddish-brownish
Koreans	reddish-violet
Poles	reddish-greenish
Kirgis	bluish-greenish

On the basis of these shades the results were classified.

This work was only of a preliminary nature since very few individuals of any particular nationality were studied. The results of this investigation corroborate the findings obtained by Manoilov. If the blood is that of a child born of parents of a racially pure marriage (father and mother of the same race), the blood will give the same reaction to the reagents as the father's and mother's only the shade will be slightly paler. The following table (table 3 taken from Poliakowa's paper) summarizes the results of the effect of one nationality upon another:

Russians × Germans	child Russian
Russians × Poles	child Russian
Russian × Finns	child Russian

Jews × Russians.....child Jew
Jews × Esthonians.....child Jew
Esthonians × Poles.....child Esthonian
Letts × Germans.....child Lett

Poliakowa considers the Manoilov method as of great practical importance, since the parentage of a child may be determined on the basis of the two following conclusions:

1. If the child's and mother's blood give a different reaction, the father does not belong to the mother's nationality.
2. If the child has a distinct race reaction which does not correspond to the mother's nationality, the father may belong to the nationality the reaction of which has been found in the child.

These results, however, are of value only in racially pure marriages.

SUMMARY

Manoilov simply by modifying various mixtures of reagents has discovered methods by means of which races, sexes and species can be differentiated chemically, according to his assertion. Although these methods convey the impression of being taken out of a cook book, nevertheless the fact remains that under certain conditions correct results may apparently be obtained. Indeed, it is Manoilov's contention that in a short time he will have a series of methods whereby all groups of animals, human beings included, can be differentiated from one another. Has he discovered a delicate test for the physico-chemical differentiation between sexes, races and species? With regard to the method for the differentiation of sexes, it has been shown that it is to a certain extent a measure of organic material present; a test involving an extremely variable factor. That different species may possess different rates of oxidation has been shown by Helff for *Amblystoma*. It may be that Manoilov detects the different rates of oxidation in different races and species by the use of his methods. But if the difference *is* a question of rates of oxidation and the rates of oxidation are correlated with metabolism, then by simply modifying the metabolic level one could obtain at one time a test indicating a male, at another time one indicating a female; or at one time a test indicating a monkey, at another a human being.

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THE BIOLOGY OF HOOKWORMS IN THEIR HOSTS

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INTRODUCTION

DURING the last quarter of a century a great deal of attention has been given to the group of parasites known as the hookworms, which infest man and certain animals. The chief reason for this attention has been the importance of these parasites in the field of public health, but out of the work have come many observations which are of interest from a purely biological point of view. Although these contributions have dealt with both the free living and the parasitic stages, it is with the latter that this paper will primarily be concerned. The chief purpose is to summarize certain experimental studies on the biology of the species of hookworms parasitic in cats and dogs. Although an attempt will be made to mention representative papers from the literature of this field, emphasis will be laid on the work of a particular group done under the general direction of Dr. W. W. Cort and supported by a grant from the International Health Division of the Rockefeller Foundation. These species found in cats and dogs furnish the most convenient material for experimental work. Although these forms are related closely to those found in man, conclusions from the experiments should be applied to the human hookworm only with the greatest caution.

SPECIFICITY OF HOOKWORMS

It will be necessary first to outline briefly the relation of the hookworms of dogs and cats to those of man and to other similar parasites of animals. As a group the hookworms are relatively specific parasites, that is, a particular species is usually found in only one species of host. There are, however, a few which are common in more than one host and most species have been found occasionally in hosts more or less closely related to their normal hosts. In addition to these recognized species differing morphologically, there are certain strains which, although identical morphologically, differ physiologically in their specificity to particular hosts.

To many the forms occurring in man are of greatest interest. The problem as to where man acquired these parasites is still unsolved. It has been suggested that he acquired them from two sources (Cameron, 1927 c). In the first place there is the possibility of acquisition of one species, *Ancylostoma duodenale*, from the carnivores. Although entirely distinct from the species now commonly found in carnivores, this human species is occasionally recorded from various members of the cat and dog families. Various species of this genus such as *A. caninum* and *A. braziliense*, common to members of these families, are found occasionally in man. On account of these facts it has been suggested that man acquired this parasite by his association with carnivorous animals in early human and not in prehuman days.

In the second place, man may have acquired certain parasites hereditarily. That is, they or their progenitors may

have inhabited the ancestor of man in prehuman days. Such a possibility has been suggested for the other hookworm of man, *Necator americanus*. Other species of this genus are found, not in animals with which man has been associated since arriving at the human level, but in primates related to the hypothesized ancestors of man. Nor is this human species found as an occasional parasite in carnivores, but the records from animals other than man are from the primates and herbivores. Furthermore, this species is less pathogenic to man than the former, and on this basis is considered by many to have been associated with man for a longer time, dating probably from prehuman days.

Of the forms occurring in animals but related to those of man, two species have been dealt with primarily in the experimental work mentioned above. The first, *Ancylostoma caninum*, is the common hookworm of dogs in the United States and many other parts of the world. It is also frequently found in cats, but as will be explained later the forms from this host are probably a physiological strain or incipient species. This species has also been reported as an occasional parasite of various members of the families *Canidae* and *Felidae* and of a few other animals. There are, moreover, a very few records of its occurrence in man. The second form, *Ancylostoma braziliense*, with which *A. ceylanicum* is now considered synonymous, is commonly found in both cats and dogs in certain localized regions scattered over the whole world, but no strain differences have yet been demonstrated. This parasite has also been reported occasionally from other Canidae and Felidae and has been found in man on several occasions, but usually in small numbers (Sarles, 1929 a). Other species of this genus are found in a variety of animals, mostly of carnivorous habit.

There are also species in various animals which are related to the Necator of man. Two species of this genus are reported from other primates and one from the domestic pig. There is some discussion as to whether this latter species, *Necator suillus*, is distinct from the human species, but it certainly seems to be different in regard to its specificity at least. Among the other genera of the same family is one species, *Uncinaria stenocephala*, which we should mention because it is the common dog hookworm of Europe and is common in foxes in America. It has also been used frequently for experimental work.

Before we enter into a discussion of the experimental work on these parasites it may be well to review the chief points in their life cycle. The eggs are passed in the feces of the host, and develop in the soil when subjected to proper conditions of temperature, moisture and soil texture. The larvae, which hatch in a few days, are infective in a week or so and seek the upper surface of the soil when there is sufficient moisture for them to exist there. Whenever they are brought in contact for a few minutes with the skin of the host, they burrow into the intact skin until they reach a blood vessel. They are then carried passively to the heart and lungs. When stopped in the capillaries of the lungs they work their way into the air passages, up the trachea to the mouth, are swallowed and carried to the intestine, where they mature. Experimentally, infestation can also be produced by ingestion of the infective larvae and this may occur in nature, especially with certain species. In the growth process in the intestine they undergo two moults. At the first of these they acquire a so-called primitive mouth capsule and at the second a definitive mouth capsule. With the aid of these and the teeth which they contain, the growing worms attach themselves to the mucosa, and live at the expense of the

host on blood and tissue. At maturity copulation occurs and the females lay enormous numbers of eggs into the lumen of the intestine over a long period of time.

ENTRANCE INTO THE HOST

The first point for us to consider in the biology of the host stages of the hookworm is obviously the entrance into the host. Skin penetration, which Looss (1911) discovered and described in detail, is probably the most common method of entrance, especially into man. Prenatal infection has been recorded as a result of larvae penetrating the fetus in the uterus after skin infection of the mother. Mouth infection may occur in any host, but probably is not common except in such a case as that of a dog eating food which has been dragged on the ground. Mouth infection has frequently been used in experimental work since the actual number of larvae entering can be determined more readily. The practice in our laboratory has been to administer the larvae in gelatin capsules so that there will be no possibility of penetration until the stomach is reached. The possibility of loss in the skin and the lungs is also eliminated by this method. Sarles (1929 d) found only about nine per cent. of the larvae given by skin developing in the intestine of pups as compared to 50 per cent. of a parallel series given by mouth.

Although there seem to be greater hazards for the larvae after they have entered the body by way of the skin, this method of entry is frequently more accessible. In such a host as man, where ingestion of contaminated food is not common, an adaptation for skin penetration is of great advantage to the larvae. Because of this adaptation there has been considerable study made of the reactions of the larvae and certain definite tropisms have been demonstrated (Brumpt, 1921; Fülleborn, 1924). It appears that the larvae react positively to heat and possibly have a histotropism which attracts them towards the body tissues. For the actual penetration the larvae need among other things something against which they can push in entering the skin. Mud caked onto the skin fulfills the requirements almost perfectly. Water drying on the skin provides a surface film which gives the proper support. The thickness of the skin probably has an effect on the ease of penetration, as does also the condition of the skin in respect to its being softened by water. It has been found, for example, that experimental penetration can be effected much more easily after the skin has been thoroughly scrubbed and exposed to a steam bath. It is apparent, however, that in nature the larvae frequently find the necessary conditions fulfilled.

Penetration of the skin calls forth certain reactions on the part of the host. A variation in the skin reaction of hosts of different ages has recently been demonstrated by the use of *A. caninum* in dogs. Sarles (1929 d) found that young dogs showed not more than a transitory inflammation after the penetration of large numbers of larvae. In old dogs, however, a marked swelling and inflammation began almost immediately and continued for a week or more with pronounced exudation and necrosis at the center of the lesion. The significance of this correlation with the age of the host will be discussed at a later place. Experimental infections of the human skin with practically sterile larvae of the species normal in man produce itching papules which subside after a few days. Typical "ground itch" as seen under conditions of natural infection is usually an open sore complicated by scratching and the action of secondary bacterial infection. An interesting variation in the skin reaction, or more prob-

ably variation in the behavior of the larvae, is the phenomenon known as creeping eruption. Recent work (Kirby-Smith, White and Dove, 1926, 1928; White and Dove, 1928) has shown that these linear advancing lesions of the human skin are due to the penetration of larvae of *Ancylostoma brazilienst*, a hookworm fairly common in dogs and cats in certain localities, and occasional in man. Why these larvae migrate just under the human skin and do not migrate under the skin of dogs, cats, monkeys and other animals has not been determined. The larvae of another dog hookworm, *Uncinaria stenocephala*, have also been shown to produce a similar condition (Fülleborn, 1928). The larvae of *Ancylostoma caninum*, however, do not produce these lesions in the human skin, but only papules similar to those produced by the human species (White and Dove, 1929).

MIGRATION TO SEAT OF INFESTATION

When the larvae have entered the body by any route they are still faced with the problem of proceeding to the final seat of infestation, namely the small intestine. After skin penetration a continuation of the boring reactions by which they enter eventually carries them to a blood vessel. Regardless of whether there is a definite reaction for entering the blood stream as has been suggested (see Looss, 1911) or whether it is a random matter, the majority of the organisms do eventually get into the blood stream, and are apparently carried passively to the lungs. When stopped in the fine pulmonary capillaries the larvae penetrate the walls and get into the air spaces. This may be due to their random penetrative movements, or they may by this time have developed a reaction specific for this purpose. In a passage through the bronchi and the trachea Looss found that there was a difference between the behavior in the dog of the larvae of the dog hookworm, *A. caninum*, and that of the human form, *A. duodenale*. He states that the larvae of the dog hookworm migrated along the surface of the mucous membrane, while those of the human species frequently penetrated the wall of the trachea. He attributes this reaction of the latter to the fact that they were in the unsuitable environment of an abnormal host, while those normal to this host were beginning to develop new reactions suitable for their parasitic existence. The fact that these larvae in the normal host started to grow at this stage will be discussed later. The passage of any considerable number through the larynx is frequently attended by a slight cough in both the animal and human host. Once in the mouth further passage to the intestine is probably purely mechanical and independent of any effort on the part of the larvae.

When the larvae are introduced by mouth and carried to the stomach it seems logical to expect development to begin immediately. The idea has been advanced, however, that the reaction for penetration causes the larvae to enter the walls of the alimentary tract and thus pass through the portal circulation to the liver, then to the heart and lungs as after skin penetration. Other nematodes such as Ascaris and Strongyloides follow this course, and it has been assumed to be true by Fülleborn (1923) and strongly upheld by Miyagawa and his associates (Miyagawa, 1913, 1916; Shirai, 1926). Yokogawa (1926), however, after a series of experiments with *A. caninum* in dogs, stated that such migration does not occur, but that development begins directly. Fülleborn (1926) confirmed this for another hookworm normal to dogs, *Uncinaria stenocephala*. The writer (Scott, 1928), working with the same form as

Yokogawa but using quite a different technique, has also found that migration does not occur regularly in dogs after oral infection, but that development continues directly.

At the same time Yokogawa introduced another phase of the problem. On the basis of experiments with the human species, *A. duodenale*, in dogs, and the dog species, *A. caninum*, in rabbits, guinea pigs and mice, he concluded that in the abnormal host this migration after oral infection does occur, although it does not occur in the normal host. In the series of experiments mentioned above (Scott, 1928) the writer used for the removal of larvae from the various organs of the host the Baermann isolation apparatus, which had previously been used for the isolation of larvae from the soil. By this method greater numbers of larvae of *A. caninum* were recovered from the liver and lungs of rats than from those organs of dogs. Cats, as will be explained later, may be considered practically an abnormal host for the strain used here. In them very few larvae were found in the lungs and liver. When, however, rats were given very large numbers of larvae by mouth a large proportion were recovered from the lungs, and this was attended by the presence of a severe and often fatal pneumonia. It has not been possible to determine whether the large numbers of larvae given were able to break some sort of resistance of the host and cause a greater migration, or whether only the usual proportion migrated but the numbers were sufficient to produce a pneumonia which prevented the completion of migration and caused an accumulation of larvae in the lungs. It seems clear then that although migration does not regularly occur after oral infection in the normal host, there are conditions under which migration does occur in certain abnormal hosts. Whether this is correlated with or dependent upon the abnormality of the host, as Yokogawa believes, does not yet seem to the writer to be clearly demonstrated.

THE GROWTH OF HOOKWORMS

One of the most interesting features of a study of the growth of these parasites concerns the point at which growth beyond the infective larval stage begins. It is at this point that development in a partially or completely insusceptible host seems to be inhibited. This raises the question as to whether growth even starts in a host in which it is never completed. Most of the experiments with these insusceptible or abnormal hosts have dealt with rodents of one species or another. Hung (1925) found what seemed to be an increase in the length of larvae passed alive in the feces of rats three days after skin infection with larvae from a mixed culture of the two human parasites. The writer attempted to repeat this with a more extensive series of experiments, using oral infections of *A. caninum*. The data, which have not been published, showed great variation from case to case so that no valid conclusions could be drawn, but the tendency was toward a decrease rather than an increase in length. About the only explanation for this change, if it were a valid one, would be that of a selection, the larger individuals having remained in the host. Measurements of larvae taken from the same animals indicate that an increase in size may have occurred in the host, but the variation from rat to rat was so great that it has not been possible to prove this to be significant. Fülleborn (1927) also found some increase in the size of the larvae of *U. stenocephala* in various rodents, but he also apparently was not satisfied that it was a significant one.

In a host where development is possible

we have a similar problem of determining when and where it begins. Looss (1911) found that in the dog development of *A. duodenale* did not begin as soon as that of *A. caninum*, although the former would develop to a certain extent in very young dogs. He reported that the first appearance of the mouth capsule of the final larval stage comes when the larvae of the dog species are in the upper half of the trachea of the dog, but he has never seen it in the lower half. The moult into this final larval stage did not occur until the intestine had been reached. Examination of many larvae from a number of cases in our series has not revealed this change taking place at this time. But Looss did not say in what proportion this occurs here, and it may be that one would expect it only rarely. In a large series of oral infections of *A. caninum* in dogs the writer found development definitely started on the third day, but never on the second. In cats no cases were found where any change from the infective larval condition was in evidence on the third day, but several cases showed some development on the fourth day.

It is a rather common occurrence that parasites may start to grow in an unusual host but never complete their development. With regard to hookworms this seems to be true in certain cases, but it is more usual that once development has started it is completed. This is shown by the fact that the number of developing worms in the host after experimental infections was found to be a constant percentage of the number of larvae given regardless of the length of time after infection (Scott, 1928). If worms were starting to develop and not completing development one would expect to find a decreasing number of worms as they grew older. This is not true until the worms have completed their growth at least. There are, however, certain cases in the literature where hookworms have been found to stop growing or to be lost from the host when only partially mature. In most of these cases, however, it has been true either that other observers have found development completed in these hosts or that the search was not continued long enough to insure that development would not have been completed.

A peculiar situation has been found to exist in regard to the growth of the dog hookworm, *A. caninum*, in the cat (Scott, 1929 c). It will be necessary to anticipate somewhat a discussion of the fact that two physiological strains of this species have been found (Scott, 1929 a). One of these is infective to dogs but only slightly infective to cats. To the other strain, however, cats are very susceptible, but dogs are almost entirely insusceptible. When specimens of various ages recovered at autopsy were measured it appeared that regardless of the infectivity of the strain, development was more rapid in dogs than in cats. The final size reached was also less in cats than in dogs by about 20 per cent. In dogs the females of the dog infective strain reached a final size of 12 millimeters in about 30 days. At this time the males had also reached their final size of 9.5 millimeters. In cats, however, the growth continued for a total of 40 days after infection, when the males and females were 8 and 9.5 millimeters long respectively. The cat infective strain apparently had an exactly similar development in these two hosts regardless of their infectivity. We are faced with the fact then that two strains of this species of hookworm which have an inherent difference in infectivity to two hosts have exactly similar growth constants in each of these hosts, but both differ with the host. We shall have

occasion at a later point to discuss more fully the significance of these facts in the biology of the worms.

It is rather interesting also that the curve of growth plotted to these data from parasitic animals was similar to that which has been fitted to a variety of free living organisms. The data show only the growth in the adult stage, that is, after the final moult. Before this moult growth apparently takes place at about the same rate as afterwards. There is probably some discontinuity at the time of moulting, but it must be slight. It has proven rather difficult to make any exact determination of the growth during the early period within the host. The time of passing through the different moults varies so much from host to host that even average figures are unreliable.

REPRODUCTION OF HOOKWORMS

In such a parasite as the ones we are considering, where no asexual multiplication takes place either outside or inside the host, there is frequently an enhancement of the only means of multiplication, i.e., the development of fertilized eggs. Biologically then the number of eggs produced is important in the preservation of the race. From other points of view also, the egg production of hookworms has become a matter of considerable interest. Among these is the importance in the minds of those studying the public health aspect of these parasites of the number of eggs produced daily. The size of an infestation harbored by a person can be estimated by a determination of the number of eggs passed daily provided there is also known the average number of eggs produced by one worm. This knowledge is valuable in turn, since it is a measure of the damage done to the patient, of the danger of the infested person to the community, and for estimating the total infestation rate of the community. The experimentalist is also interested in the egg production, since it furnishes a convenient measure of the size of an infestation.

Sexual maturity may be determined by two methods. In the first place copulating pairs found at autopsy indicate that it does not occur before the twelfth day in *A. caninum*. Determining the time of appearance of eggs in the feces of the host is another method which does not involve the death of the host. In A. *caninum* this has been found to occur on about the fourteenth or fifteenth day after infection. Herrick (1928) states that this prepatent period was longer in a group of older dogs. In cats (Scott, 1929 c) infested with this same species, eggs did not appear in the feces before the twentieth day. The period averaged 22 days in a number of the latter host. In *A. braziliense*, however, this period was about the same for dogs and cats, averaging 15 days in young animals of either species (Sarles, 1929 a). In old cats, however, the average period was longer, namely 20 days. Old dogs have not been tried extensively. The species parasitic in man take much longer to mature, namely about 40 days.

Determination of the number of eggs laid by the female worms in a given period of time is complicated by a number of factors. In the first place it must be based on counts made of the eggs produced by all the worms in a host. If one could be certain that only one female were present throughout the period studied, the matter would be considerably simplified. The egg output of larger infestations has been shown (Herrick, 1928; Sarles, 1929 c) to rise for a short period of time and then decline. Although the rise is probably due to increasing egg production of the individual worms, it will be shown later that the decrease is probably mostly due to the death of some of the worms. Be-

sides such a variation in the egg production of the worms with their age, it may also vary with the species, size and age of the host. Sarles (1929 c) has shown that the number of worms present in an infestation affects the egg output, there being smaller numbers of eggs produced by the females when present in large infestations than when present in small ones. Besides these variations in the actual egg production of the female worms, there are other variations, such as the output of feces of the host, which make its determination difficult.

As mentioned before, the actual estimates of egg production have assumed considerable importance in public health work, since, once this factor is determined, the process can be reversed and the number of worms present in an infestation can be estimated from the egg output. Estimates of the number of eggs produced daily by females of *Necator americanus* are usually between 9,000 and 10,000 (Stoll, 1923; Sweet, 1925; Hill, 1926). The daily egg production of *A. duodenale* is estimated as considerably more, namely from 20,000 to 25,000 (Soper, 1927; Cort, Stoll and Grant, 1926; Augustine *et al.*, 1928). These estimates from human infestations are based on average natural infestations supposedly containing worms of a wide variety of ages. In the species infesting animals the age can be controlled more exactly. Sarles (1929 c) found that the daily egg production of female *A. caninum* in dogs increased as mentioned above for the first month up to about 20,000 to 25,000 eggs per day. In heavy infestations (over 200 worms), however, this value did not go above 10,000. From this time on he found a decreasing egg production. Herrick (1928), on the other hand, found that in a series of dogs infested with the same strain of worms, the average egg production increased from 7,000 to 17,000 over a period from 30 to 200 days of age. This difference may possibly be due to the fact that he was dealing with lighter infestations in the older ages.

The length of time over which egg production continues and the length of life of the worms are also difficult matters to determine. This is true mainly because it is absolutely essential to demonstrate that no subsequent infection occurs after the experimental one. Furthermore, about all that can be determined with certainty in regard to the species infesting man is the maximum length of life. Various estimates and determinations have been made but are often invalidated by the fact that the nonoccurrence of reinfestation was not completely assured. About all that is certain is that the maximum life of either species is not usually less than three or four years. Chandler (1925) attempted to measure the length of life indirectly and found that the average egg count of inmates of a well sanitated jail decreased 50 per cent during the first few months after admission. The relation between this determination and the maximum length of life is again not fully known. Sarles (1929 b) found the maximum length of life of *A. caninum* in dogs to vary from 43 to 100 weeks. He found, as had Herrick (1928), that egg output of an infestation decreased rapidly after the first month. Although some of this decrease may have been due to a decreasing production of eggs by the worms, as was mentioned above, it was probably primarily a result of the death of worms. On the basis of these results it might very well be that although Chandler did find such a rapid decrease in egg output in human infestations, maximum length of life was a matter of years.

THE EFFECT OF HOOKWORMS ON THEIR HOSTS

Because of the importance of the disease caused by hookworms there has been a great deal written in regard to their effect on the human host. No attempt will be made here to relate any of the clinical pictures caused by the parasites, but a few of the outstanding points in their effect on laboratory animals will be mentioned and compared with similar effects in man. Chronic hookworm disease causing debility, anemia and loss of weight, such as is common in man, does not appear to be so frequent in dogs. In our laboratory this picture has only seldom resulted from experimental infestations, and these cases were not all entirely uncomplicated. The most common result of an infestation large enough to produce a noticeable effect on the health of the host has been death, usually occurring on about the twelfth day. This is at the time when the worms are growing most rapidly and is probably correlated with blood loss, as will be explained later. Other dogs which had received a few less worms or whose resistance was slightly greater showed symptoms of exhaustion for a few days at about this time and then rapidly recovered and appeared entirely normal throughout the rest of their lives. Visitors to the laboratory have repeatedly been unable to distinguish such dogs from uninfected controls from the same litters. Apparently on an adequate diet a dog is usually able to compensate entirely for the damage done by the worms once it has passed the critical period during the rapid growth stage of the worms. As already mentioned there have been cases where the damage has been chronic, and the dog has been in poor condition for a long period of time. This is the type of damage most familiar to veterinarians, since acute cases would not so often be brought to their attention. Furthermore, the latter type might not occur very frequently in nature, since there the larvae are probably acquired gradually.

Attempts to measure the damage caused by hookworms have not been attended by a great deal of success. The chief quantitative results have been from studies of blood changes. Many of these studies have been on anemia, since this is the most outstanding feature of the clinical picture of hookworm disease. In the first place the cause of the anemia has received considerable attention. There are three ways in which the anemia may be caused: chronic blood loss, hemolysis and destruction of corpuscles in the blood stream, or a toxic effect on the blood forming organs.

The loss of blood has been calculated at various times, and there seems to be a consensus of opinion that it is not sufficient to cause the effects seen. In the acute cases in dogs mentioned above, however, it can hardly be doubted that blood loss was the important cause of the anemia. The intestine at autopsy was greatly injected, the mucosa bright red and covered with blood, and the lower part filled with clotted and partly digested blood. The stools for several days before death were composed only of blood and mucus. The red blood cell count (Sarles, 1929 e) diminished so rapidly as to suggest that the effects were due to blood loss rather than an effect on the blood forming organs.

This sort of blood loss is not the common picture in chronic cases, however. Sarles (1929 e) shows that the red cell count and hemoglobin of a chronic case in a dog declined and persisted at a level lower than normal. This is apparently similar to the condition in most chronic human cases. In man it has not been possible to follow the blood picture of a chronic case over any considerable period of time, but a large number of determinations of hemoglobin have been made. In practically every case the

hemoglobin index of even moderately severe cases averaged lower than that of non-infested individuals of the same community. The literature on certain phases of this problem has been recently reviewed by Cort, Schapiro, Sweet, Stoll and Riley (1929). This chronic anemia is the picture which does not seem to be adequately explained on the basis of blood loss. Neither do explanations based on hemolysis and destruction of red corpuscles have many advocates. Among other arguments are those of De Langen (1922) who has shown that the bilirubin in the blood is not above normal as it is in chronic malaria; and that the red corpuscles are not excessively fragile.

The remaining explanation is that of a toxic depressing action on the bone marrow and other blood forming organs. The pathology of these organs in chronic cases has not been adequately studied. Furthermore, there is still a possibility that this action is not due to the hookworms themselves, but to a secondary bacterial invasion in the wounds they cause.

Most of the other blood changes are of doubtful significance. The white count is usually high, although it may be below normal in long-continued severe cases. The eosinophiles, however, respond to the invasion of hookworms as they do to many animal parasites. Sarles (1929 c) has made a study of eosinophila in dogs in order to throw light on the scattered facts recorded in the literature concerning human cases. He found marked changes in adult dogs infected either orally or cutaneously with *A. caninum*. Along with a general leucocytosis there was a pronounced eosinophilia going as high as 42 per cent in one case and persisting for some time. This rise was preceded, however, by a decrease in the number of eosinophiles in the circulating blood which may indicate a local accumulation and a delayed response of the organs where they are formed. In young dogs, while there was a similar general leucocytosis, the changes in the eosinophiles were distinctly less than in old dogs and appeared to be associated with the production of anemia.

BIOLOGICAL VARIATIONS IN HOOKWORMS WHICH ARE CORRELATED WITH VARIATIONS IN THE HOST

There are a number of variations in the biology of hookworms which are closely correlated with variations in the host, and which appear at least to be the direct result of the latter. Variations may be due to the fact that one species of parasite may frequently be found in more than one species of host, or they may be correlated with the age of the host. Among other host variations which may be suggested but about which little is known are differences in the size of the host, its condition of health, its diet, and the presence of other parasites and diseases.

One of the most interesting cases of variations correlated with host species is found in the situation regarding the hookworms of domestic dogs and cats. This is especially interesting because it has permitted a certain amount of experimental analysis. Two species of the genus Ancylostoma are commonly found in these hosts. *A. braziliense* is frequently found in both dogs and cats in certain localities over many parts of the world. It is apparently indiscriminate in regard to these two hosts and passes readily from one to the other. The other species, *A. caninum*, behaves quite differently when tested under controlled conditions. From the results of experimental infestations with the two hosts (Scott, 1929 a) it is apparent that this species does not pass readily from one host to the other.

Larvae from infestations in dogs do not easily infect cats, and it is scarcely possible to infect dogs at all with those from cats.

An attempt has been made to find out something of what this situation means in the biology of this parasite. The most satisfactory hypothesis seems to be that the species *A. caninum* was originally a parasite of dogs. The genus is composed, however, of various species of parasites of both cats and dogs, so that it seems probable that it was also slightly infective to cats. Then by some means a modification occurred in a certain portion of the species so that it became more infective to cats. Apparently in doing this it also became less infective to dogs, so that we now have in existence the two types of strains. A study of the distribution of the species shows that it is much more common in dogs than in cats. Where it is reported for cats it usually occurs in small numbers and relatively infrequently. In certain localities, however, it does appear to be fairly common in cats and occurs in large numbers. A test of one such locality (Scott, 1929 b) showed that worms from cats represented cat infective strains. At the same time a test was made of worms from dogs from a locality where the species occurs in dogs but not in cats. In this case all the strains tested proved to represent strains highly infective to dogs but only slightly to cats. There seem to be at least two possible explanations of how these modifications took place. Although this species is rather rigidly specific to dogs and to cats as explained, it has been recorded occasionally from a large number of other hosts, mostly *Canidae* and *Felidae*. If then we start with the hypothesis that the species was originally a parasite of dogs, it would still be possible to consider that it occurred in other hosts occasionally.

Since man's domestication of animals and possibly earlier, the domestic cats and dogs have been more closely associated than any other members of their families. This species of hookworm, *A. caninum*, would probably occur then in cats more frequently than in any other host besides its original one. Now it is a possible hypothesis that if the parasite existed and reproduced in a new host for one or more generations, it would become better adapted to that host. On the other hand, it is possible that the occasional specimens found in hosts other than the original one represent variants whose infectivity has been suddenly changed by mutation or otherwise. When both sexes of such variants occurred in a new host, the progeny would represent a new strain infective to that host. Which of these two explanations is true cannot be stated at the present time. Certain further experiments (Scott, 1929 c) have been done to test them, but no final conclusions have yet arisen. Two of the typical strains were introduced into the hosts to which they were not highly infective and the infectivity of their progeny tested after one or more generations. The experiments were not extensive enough to warrant definite conclusions, but it seemed that the dog infective strain did not become any more infective to cats or less so to dogs by having passed from one to three generations in cats. On the other hand the cat infective strain did appear to become more infective to dogs and less so to cats by having passed only one generation in dogs. If our hypothesis that this species was originally a parasite of dogs is correct, it appears that modification to the cat infective condition may have been difficult to achieve or occurred infrequently, but the reverse modification of a cat infective strain back to a dog infective condition was relatively easy. These experiments do not eliminate, however, the possibility of

the change being one of a genetic nature. Other experiments (Scott, 1919 c) have been tried to test this by attempting to produce a cross between the strains in both animals, but so far it has not been possible to do so. The experiments are not extensive enough to determine whether it will ever be possible or not. Other genetic experiments, which will determine the correct explanation of the modification of infectivity, are undoubtedly feasible and should be attempted.

However these variations may have occurred, the facts indicate that at the present time this species, *A. caninum*, occurs in two types of strains which have differing inherent infectivity to two species of hosts. The local distribution of the cat infective type of strain indicates that it has probably originated from the dog infective type at various places and times.

In contrast to these inherent variations in the strains, the characteristics of growth of both types of *A. caninum* are apparently entirely independent of any modifications of infectivity but are dependent on the species of host. Growth does not occur as rapidly and the worms do not reach as great a final size in cats as in dogs (Scott, 1919 c). This is true for both strains regardless of their infectivity. We may assume in connection with our hypothesis that the species was originally a parasite of dogs, that the cat is not as suitable a host as the dog, and for some reason growth is somewhat inhibited in the cat. Even though certain strains have been developed to which the cat is a more suitable host from the point of view of susceptibility, the influence on inhibition of growth is still present. It would be interesting to see how this influence of the host species on the growth would be reflected in the egg production and in other functions.

When we turn to the other species of this genus common to cats and dogs we find quite a different situation. *A. brazilienſe* shows no evidence that it was ever primarily a parasite of one species of host or the other. No differences in strains have been detected, and larvae from infestations in either host are infective to both species. Growth has not been measured adequately, but a few observations are recorded (Sarles, 1919 a). These available measurements indicate that growth may take place slightly faster in dogs than in cats, but the difference is not significant, and the similarity of the prepatent periods in the two hosts shows that there can be no such difference as is the case with *A. caninum*. Although we have no evidence to explain how it occurred, it seems that this species, *A. brazilienſe*, has become adapted equally well to life in cats and dogs.

Another important variation in the host which influences the parasite is the age of the host. We have continually mentioned various influences of this factor, and it may be well to recapitulate them here. The most noticeable effect is what has been called age resistance. With the species *A. caninum* and *A. brazilienſe* in both cats and dogs there is a definitely decreasing susceptibility correlated with advancing age of the host. As an example there may be cited variation with the age of the host of the percentage of larvae given by mouth which develop to maturity. Herrick (1918) found that in young pups an average of 35 per cent. of the larvae of *A. caninum* developed, while in mature dogs only three per cent. developed. This was the dog infective strain, which is scarcely infective to cats, but even with this, more developed in kittens than in mature cats (Scott, 1918). The cat infective strain (Scott, 1919 a) showed the same difference more markedly

in this host, an average of 45 per cent. developing in kittens, while a much smaller percentage developed in cats. With *A. brazilienſe* the same was true in both cats and dogs. Sarles (1929 a) reported that an average of 32 per cent. developed in a series of young cats while only four per cent. developed in adult cats. Likewise he found a correlation with the age of dogs, 44 per cent of the larvae developing in puppies and five per cent in adult dogs. Although such exact experimental work cannot be done with the human host it seems fairly obvious that no such correlation with age can be true for either species infesting man. Epidemiological studies have shown that in practically all regions children have fewer hookworms than adults. In the few cases where children are more heavily infested than adults, the difference seems to be correlated with the habits of the two age groups. Many extremely large infestations do occur in adults, which would be quite unlikely if there were such a difference in susceptibility with age as is found in the other species.

Other effects correlated with the age of the host have been noted. The skin reaction of old dogs, in contrast to that of young dogs infected with larvae of *A. caninum*, has already been described. Although further work along this line is needed, it seems almost a certainty that this difference was correlated with the age of the dog and not with previous infestation. It has also been mentioned that in the same series (Sarles, 1929 e) the old dogs showed an eosinophilia much more marked than that in young dogs. A study of the growth of *A. caninum* showed no correlation between the length of the worms and the age of the host, although it is not improbable that more extensive series might do so (Scott, 1929 c). On the other hand, both this species (Herrick, 1928) and *A. brazilienſe* (Sarles, 1929 a) seem to require longer to come to sexual maturity in old than in young hosts. There are so many other variables to consider in a study of egg production that it has not been possible to determine whether this function varies with the age of the host.

IMMUNITY TO HOOKWORMS

That the substitution of the words susceptibility and insusceptibility for the terms immunity and resistance in referring to animal parasites is a practice to be preferred, is an opinion which the writer has previously expressed (Scott, 1928).

> The word immunity, for example, has for the most part, been applied in bacteriological work to that phase which has so far been of the greatest medical significance, the development of acquired immunity. . . . . Furthermore the word resistance is apt to produce the unconscious impression of the presence of factors in the host actively antagonistic to the parasite or its effects.

There is another caution which should be observed in using terms which are common in bacteriological parlance, that is we must clearly distinguish between the insusceptibility to the effects of parasitism and the insusceptibility to the colonization of the host by the parasite. The writer has mentioned this matter before (Scott, 1928) and can do no better than to repeat.

> In the case of parasitism with most helminths it is possible to distinguish quite readily between colonization of the host by the parasite on one hand, and the effects of parasitism on the other. In bacteriological work this distinction has frequently been omitted. From a medical point of view the effects on the host are often of predominant interest. In many cases it is difficult to distinguish between resistance to colonization and to its harmful effects. Cases of the natural insusceptibility of a species or race to the toxic products of bacteria are known. Kolmer (1923) cites a number of cases where antitoxin apparently exists in an insusceptible species of animal, and where toxic products, when produced, find no suitable receptors in

the cells of the host. Schwartz (1921) has shown that *A. caninum* produces a nonspecific hemolysin and that normal serum has antilytic properties which inhibit the action of this hemolysin. It may be then that the situation in regard to the susceptibility of the host to the effects of the parasite will be found to be quite parallel in bacteriological and helminthological parasitism. Our interest is, however, not so much in the insusceptibility of hosts to the effects of parasitism as in their susceptibility to colonization by the parasite.

There are several kinds of reactions which may come under the term insusceptibility to infestation with regard to the hookworms. In the first place, there is what may be called racial immunity or species insusceptibility. We have seen that the hookworms are relatively specific. In other words only one or two hosts are ordinarily suitable for the development of each species, but rarely a number of other hosts may be sufficiently suitable to allow a few to grow. It seems as though a knowledge of what happens to larvae introduced into these hosts in which they do not ordinarily develop might possibly help to explain the mechanism of other types of immunity as well. One fact of considerable importance has been learned from work which the writer has been pursuing along this line. If the larvae which are introduced into a host are unable to grow they tend to remain in the host in the same condition in which they were introduced. The significance of this phenomenon will be discussed later, since it has been found to occur also in other types of insusceptibility.

Insusceptibility correlated with the age of the host has already been discussed in the previous section, and we may consider next the immunity due to the presence of a previous infestation. Carefully controlled experiments to determine whether there is any inhibition to the development of larvae introduced while a previous infestation is still present are greatly needed. Those which have been recorded are somewhat inconclusive on this point. Herrick (1928) found that a second infestation given while the first was still present was just as large in proportion to the number of larvae given as was the first. In some cases infestations did seem to be a little smaller when two or three previous infestations had been given and were still present. The method of measuring the size of the infestation based on the egg output was quite variable, and he interprets his results with caution. Decreasing susceptibility due to advancing age of the dog during such an experiment is apt to mask the result. It is certain, however, that if such an insusceptibility is demonstrated it will not be absolute and probably will not be of any great degree.

Another consideration which has frequently been mentioned is whether insusceptibility to infestation results from the presence of worms of other species. The question has been raised in a number of cases where the facts cannot be easily explained on other grounds. Sarles (1929 a), for example, records a case of a dog which failed to take an infestation of *A. braziliense* which on the basis of previous work should have been established. The presence of a large infestation of *A. caninum* was the only factor which differed from other cases in the same series which did take an infestation. Other similar cases frequently come to notice and indicate that a series of experiments should be performed to clear up the question as to whether this insusceptibility is due to some extraneous factor, or in case it is due to the presence of another species, how frequently it occurs and under what conditions.

What is usually termed acquired immunity should be distinguished from the above immunity due to an existing infestation,

since in this case the insusceptibility arises from the fact that the animal has had a previous infestation, but it is no longer present. We have no evidence that there is any such phenomenon in the case of the hookworms. In a series of experiments Herrick (1928) showed that there was no reduction in the size of an infestation of *A. caninum* established in dogs where previous infestations had been present but cleared out by anthelmintics.

THE MECHANISM OF IMMUNITY

No study of immunity or insusceptibility is complete without a consideration of the mechanism involved. Few immunological mechanisms have been carried beyond the stage of hypotheses. In the case of insusceptibility to parasitic worms no widely accepted hypothesis has been proposed and very little has been done toward establishing any hypothesis that has been suggested. Preliminary to a study of the mechanism of immunity there must be a determination of the conditions under which various types of insusceptibility occur. The hookworm problem is rapidly reaching the point where it is safe to consider that these conditions are roughly outlined and already some observations bearing on an explanation of the insusceptibility have been made.

There is a possibility that a local immunity is developed in the skin of animals which prevents penetration twice in the same area. Moreover, it is conceivable that this immunity could spread from a localized area and cover the whole skin. Its action would probably not be that of preventing actual penetration, but that of killing or inhibiting the larvae after they were in the skin. No extensive attempts have been made to determine whether such an immunity exists. Sarles (1929d) found a peculiar situation in old dogs insusceptible to infestation and in which the marked cutaneous reaction described above followed the penetration of the skin. From these areas live larvae were recovered for a period of 60 days in some cases, and in all cases they persisted much longer than in young dogs in which no cutaneous reaction occurred. Sections of the area showed that many larvae were present, some apparently alive, and some dead and surrounded by a cellular infiltration. Further histological work on experimental animals should help elucidate the mechanism of any cellular reaction which may be important in whatever immunity is developed. It should also be possible with the proper technique to demonstrate the presence of any antibodies which might be developed against the larvae.

It has been repeatedly demonstrated (Scott, 1928; Sarles, 1929 a) that after an infection by skin or by mouth of even entirely insusceptible hosts, the larvae can be found alive in the intestine. This was made possible by the use for recovering larvae from various tissues of the host of the Baermann isolation apparatus, which was previously used for isolating larvae from the soil. The most fundamental point in the immune reaction must come in the intestine, and the critical point for the parasite is in being able to initiate development. Certainly it is not primarily a matter of the insusceptible host destroying the parasites. A series of experiments involving both normal and abnormal hosts (Scott, 1928) have shown that regardless of whether few or many worms developed, there was usually a residual number which did not. These larvae which did not develop were recovered alive in considerable numbers from the same place as were those which did develop. For example, a cat was infected with larvae of the dog strain of *A. caninum*. After 44 days the animal was killed and found to harbor a few mature worms

which had resulted from the infection given. At the same time larvae were recovered from the intestine entirely undeveloped and in exactly the same condition they were in when they were introduced. Similar larvae were found in rats where none were developed, and in dogs where a large number had developed.

The question which now presents itself is: Why did these larvae not grow? In some cases from the same group of larvae introduced into various animals, many were found undeveloped in rats and none developed, in cats a few were developed and many undeveloped but alive, while in dogs most of those given were developed but a few were unchanged. Why did varying numbers grow in different hosts? Certainly not because of a difference in the larvae, for they were all from a common source. Furthermore, it was demonstrated that those which did not develop were not incapable of development. They were removed from these various hosts and introduced into other hosts of the same or different degrees of susceptibility. Here the percentage development was exactly the same as would have been postulated for the particular host infected with newly cultured larvae. Even here, however, a certain percentage would not develop, but remained unchanged as before. In one case some of this group were again transferred to a third host and the usual percentage developed. In other words the larvae were apparently all potentially able to develop, but certain host conditions determined what percentage developed in each case. What are the necessary conditions in the host, is now the crucial question. It may be that the host inhibits the development of all but a certain number of the larvae. Or it is just as likely, or perhaps more so, that certain peculiar conditions are necessary for development and the degree to which these conditions are fulfilled determines the percentage which can develop.

This hardly seems to be anything like the situation in bacteriological immunity, where certain antibodies are supposed to prevent the normal growth of the organisms. Here we have one host in which none of the parasites grow, another in which few grow and still another species in which many grow. In all of these, however, some of the parasites do not grow, but they do remain alive in the host and are entirely capable of development if transferred to another host. In hosts where susceptibility varies in accordance with age the same phenomenon occurs, and again the situation is still more complicated by the fact that the conditions in two hosts are exactly reversed when a different strain of parasite is used. The situation is not simple, then, and it hardly seems possible that any hypotheses which have been suggested for other organisms will account for these facts.

PRESENT PROBLEMS IN THE BIOLOGY OF HOOKWORMS

At every point in our discussion there have arisen many unsolved problems, some of which may now be collected here together. We have seen that a great deal of information has been gathered in regard to the distribution of the various species and their specificity to various hosts. But continued observations will undoubtedly bring to light new facts, and especially should the distribution of various host strains be more adequately studied.

Although skin penetration was discovered so long ago and a great deal of work done since, we are still far from being able to explain all of the biology of the larvae involved in skin penetration. The reaction of the host to skin penetration is connected so closely with what may be immune reactions that we may consider

the former to be a part of the latter problem. Migration from the skin and after oral infection is not adequately described. Why do larvae remain in the skin longer in old dogs than in younger ones? Do they also remain longer in the skin of other insusceptible animals? We have seen that after oral infections migration does not usually occur in normal hosts, but in abnormal hosts it has been recorded under certain conditions. What are the factors which cause this difference? Do they have any bearing on the subsequent development of the parasite? We have seen that the study of the growth of hookworms has demonstrated that in the adult stage they follow growth curves similar to those of free living animals. The most significant variation is apparently due to the species of the host. This should be investigated to determine the mechanism involved. Furthermore other variations possibly correlated with variations in the susceptibility of the host will doubtless appear. A study of the growth during earlier stages is difficult technically, but not impossible, and may give clues to other biological facts of importance.

Egg production will undoubtedly be studied further. It is our sole method of estimating the size of infestations. Its importance to experimental and practical work will require that the variations be more adequately defined. Although these practical considerations may be uppermost in the mind of the investigator, our knowledge of the biology of the parasite is bound to advance.

We have only begun to study the effect of hookworms on the host. Even the pathology of well recognized clinical manifestations is relatively unknown and future work involving the pathology of the physiological reactions of the host ought to be fruitful. Furthermore, little has been done toward studying the balance of relations between the host and the parasite. We know only a few variations in the parasite which seem to be caused by variations in the host, and practically nothing of the factors involved. The relation of the physiological strains already described and the other possible ones offers a very attractive field. If it is a genetic matter, an experimental attack ought to elucidate the problem. If it is not, an attempt to produce modifications should be of great value.

The immunological relations of the host and parasite are also practically an untouched field. In the first place we need to know more of the facts of insusceptibility. Is there any immunity due to the presence of a previous infestation? Is there any due to a previous infestation which has been lost? How rapidly does the insusceptibility increase with the age of the host? Then the mechanism is entirely unknown. As already pointed out, significant results will undoubtedly come from the application of methods which have been useful in bacteriological immunology. The facts known at present, however, indicate that the inhibition through the production of antibodies may not be as important as it is in bacterial reactions. We certainly know that hookworms are not killed by the insusceptible host, but remain in it alive and capable of development. What are the factors necessary for this development? This is probably the most crucial question we have to face at the present time.

This paper is a contribution from the Department of Helminthology of the Johns Hopkins University, School of Hygiene and Public Health, supported by a grant from the International Health Division of the Rockefeller Foundation. The writer wishes to thank Dr. W. W. Cort and members of his department for aid in consultation and criticism.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

EMERGENT EVOLUTION AND THE INCARNATION. *Washington Cathedral Series.*
By William C. Woods.
Morehouse Publishing Co.
$1.00 *Milwaukee*
4½ x 6¾; x + 109

Here is a striking proof that in this generation the children of light are as up and coming as the children of this world: emergent evolution, now so fashionable among philosophically minded biologists, serves in this book as a buttress to orthodox Christian theology. Dr. Woods knows far more about theology than most biologists and far more about biology than most theologians. If then his argument is not altogether convincing, the fact may be taken as an index of the intellectual difficulties of reconciliation.

Like most writers on the subject, Dr. Woods considers the virgin birth of Jesus as a unique event. This is by no means justifiable. There are a number of cases of virgin birth in the literature, some of them in America. Thus among the Algonquins Manibozho, among the Hurons Joskeha, among the Mexicans Quetzalcoatl and Uitzelopochtli, among the Peruvians Viracocha, were all virgin born. Yetl, a tribal hero of the Thlinkeets of North Western America, was conceived as a result of his mother's swallowing a pebble and a draught of seawater on the advice of a friendly dolphin. The conception of Jurapari of the Brazilian Uapès was the result of his mother's drinking fermented liquor. In many other instances conception has followed the use of fermented liquor, but these are not properly speaking cases of virgin birth, the liquor not being the proximate cause of the conception.

WHAT IS DARWINISM?
By Thomas Hunt Morgan.
W. W. Norton and Co., Inc.
$1.00 4⅛ x 6½; 78 *New York*

In this reprint, with additions, of his article in *The Yale Review* Morgan considers the bearing on the theory of natural selection of later developments in genetics, especially Johannsen's work on pure lines and the various researches on mutation. While admitting the force of Bateson's objection that infertility has not been observed between a mutant and its parent

type, he concludes that Darwin's argument for organic evolution "is as cogent and convincing today as it was nearly seventy years ago." In the second part of the book Morgan replies to J. S. Haldane and A. V. Hill on the sufficiency of a mechanistic interpretation of self-regulating organisms. In these days of triumphant mysticism it is pleasant to find a distinguished biologist who is still a staunch mechanist.

THESE MISSING LINKS; *Comments on Human Evolution.*
By Francis P. LeBuffe. The American Press
10 cents 5 x 7½; 32 (paper) *New York*

This little pamphlet contains a collection of short papers in which the author, a member of the Society of Jesus, attacks the views of those anthropologists and ethnologists who seek to prove the evolution of the human species from lower forms. He refuses to see any evidence that in primitive man, either of existing tribes or in ancient remains, can be found any of the so-called missing links between the higher anthropoids and *Homo sapiens.* He quotes freely from the writings of Osborn, Gregory, Hřdlička, Burkitt, Elliot Smith and many others, including a number of Catholic writers. Naturally, he carefully selects his material to prove his point.

ORIGIN THROUGH EVOLUTION.
By Nathan Fasten. Alfred A. Knopf
$3.75 trade edition 5⅛ x 8; 456 *New York*
$3.00 text edition

A pedagogic presentation for the layman and beginning student of the general idea of evolution in nature and the evidence which supports its acceptance by scientists. Both organic and inorganic evolution are dealt with and modern physical theories and general biology reviewed on the way. So large an undertaking in so limited a space is necessarily bound to be somewhat superficial. However, for its intended purpose, an elementary textbook, it is excellent. Glossary and index are provided.

GENETICS

PLANT HYBRIDIZATION BEFORE MENDEL.
By H. F. Roberts. Princeton University Press
$4.00 5⅜ x 8⅜; xiv + 374 *Princeton, N. J.*

This volume will be extremely useful to all students of genetics. The title is somewhat misleading. While a large part of the book is devoted to pre-Mendelian work, the author includes the work of Gregor Mendel and of those investigators who rediscovered his law. The presentation of the subject is historical rather than genetic in character. The aim of the author is to present the work of all investigators who have contributed in some manner to the theory of fertilization and hybridization in plants. Besides very thoroughly analyzing the important researches in this field he includes many quotations from the original papers. He thus brings into relief the important steps in the development of the genetic theory and rescues from obscure sources many interesting details of historical interest which might otherwise be lost sight of. Special chapters are devoted to the researches of Kölreuter, Darwin, Galton, Mendel, and Bateson, while in chapter XI will be found an excellent presentation of the subject matter in the papers of DeVries, Correns and von Tschermak on the rediscovery of Mendel's law. Added interest is given the book by numerous

illustrations, including photographs of many of those who have been pioneers in the hybridization of plants. Each chapter concludes with a list of references. The book is well indexed, while under a special heading is given a list of hybrids by genera of their parentage, and of genera made the subject of experiment.

HANDBUCH DER VERERBUNGSWISSENSCHAFT. *Lieferung 6. Band I. Fortpflanzung und Befruchtung als Grundlage der Vererbung.*
By Max Hartmann. *Gebrüder Borntraeger*
9.60 marks (Subscription) *Berlin*
19.20 marks (Single copies)
7 x 10⅜; 103

HANDBUCH DER VERERBUNGSWISSENSCHAFT. *Lieferung 9. Band II. Verteilung, Bestimmung und Vererbung des Geschlechts bei den Protisten und Thallophyten.*
By Max Hartmann. *Gebrüder Borntraeger*
12 marks (Subscription) *Berlin*
24 marks (Single copies)
7 x 10⅜; 115 (paper)

HANDBUCH DER VERERBUNGSWISSENSCHAFT. *Lieferung 7. Band II. Arthastarde bei Pflanzen.*
By O. Renner. *Gebrüder Borntraeger*
14 marks (Subscription) *Berlin*
28 marks (Single copies)
7 x 10⅜; iv + 161 (paper)

The first two of these parts of this great handbook of genetics, by one of the editors, Dr. Max Hartmann, put heavy emphasis on the cytological aspects of the problems of reproduction and fertilization, and the determination and inheritance of sex in primitive plants and animals. They are abundantly illustrated and carry sufficiently extensive bibliographies. Renner's part on species hybrids in plants is an extremely thorough, interesting and useful review of knowledge in this field, in which he is an outstanding worker.

GENERAL BIOLOGY

GENERAL SCIENCE *for Reviews.*
By W. Dean Pulvermacher and Charles H. Vosburgh. *Globe Book Co.*
50 cents 5 x 7½; 168 (paper) *New York*

This textbook for general science courses in elementary and secondary schools gives in "a simple clear style, not beyond the intelligence of the average student," a mixture of physics, chemistry, astronomy, meteorology, physiology, botany and hygiene. Here the unsuspecting child learns that alcohol

(1) Lessens physical activity. (2) Lessens skill. (3) Decreases resistance to disease. (4) Increases nervousness. (5) Has a harmful effect upon the offspring. As to its effect upon the separate organs of the body (1) it causes the digestive juices to flow more freely at a time when not needed, (2) it burns the lining of the stomach, (3) decreases the effectiveness of the white corpuscles, i.e., of their ability to resist bacteria, (4) hardens the walls of the arteries (arteriosclerosis), (5) causes the brain center that controls the smallest arteries to lose its control over these arteries. They relax and flood the capillaries going to the surface of the body with blood. This gives the effect of warmth but the body is really losing heat too quickly, (6) the skin is thus flooded with blood and doesn't act properly as a regulatory organ, which it ordinarily does by allowing more blood to the surface in order to pour perspiration on the skin when one is too warm. If one is too cold the tiniest arteries to the surface are closed and blood is not allowed to get to the skin. The center controlling this action is numbed by alcohol, (7) it causes hardening or cirrhosis of the liver, (8) the nervous system gradually breaks down, the higher centers involving the better traits first and finally in the last stages delirium tremens, (9) the muscles are keyed up to a greater effort for a short time but the reaction is such that the average work done is less, and it is for that reason and because of the fact that the muscular ability will soon be lessened, that trainers do not allow athletes to drink alcohol.

Demonstration. To show the effect of alcohol upon protein add some alcohol to white of an egg and note the hardening effect, then add alcohol to spirogyra plant and note that when placed under the microscope the protoplasm within the cells is contracted.

That alcohol, in the dilutions commonly ingested by civilized persons, might have a different effect than when poured undiluted on egg albumen is not suggested.

LABORATORY AND FIELD ECOLOGY. *The Responses of Animals as Indicators of Correct Working Methods.*
By Victor E. Shelford.
The Williams & Wilkins Co.
$10.00 6 x 9; xii + 608 *Baltimore*

In this book Professor Shelford has made a monumental and encyclopedic contribution to the methodology of animal ecology. It was begun ten years ago with the intention of describing the author's equipment only. Since then the plan has broadened, until in the end more than half the book is devoted to the presentation of ecological data and results. The range and thoroughness of the treatment is indicated by the chapter headings: Some problems of ecology and climatology; methods and results of biotic observation and experimentation; behavior and acclimation; food and food effects; soil and the growth of food plants; dormancy and other quiescent states; animals in relation to temperature; control and measurement of temperature; ventilation under experimental conditions; control and measurement of moisture; temperature and humidity in combination; the effect and measurement of air pressure and evaporation; light conditions and effects; measurement and control of light; the evaluation of factors other than temperature and moisture, and the comparison of species; location and planning of buildings and equipment for the simulation of climatic conditions; physical conditions in water (except light); light conditions in water; chemical conditions in water; location and planning of buildings for the simulation of aquatic conditions.

There is a bibliography of over 1000 titles, and a detailed index.

A BIBLIOGRAPHY OF AMERICAN NATURAL HISTORY. *The Pioneer Century, 1769–1865. The Rôle Played by the Scientific Societies; Scientific Journals; Natural History Museums and Botanic Gardens; State Geological and Natural History Surveys; Federal Exploring Expeditions in the Rise and Progress of American Botany, Geology, Mineralogy, Paleontology and Zoology. Vol. III. The Institutions Founded or Organized between 1845 and 1865. Bibliography of Books. Chronological Tables. Index of Authors and Institutions. Addenda to Volume I.*
By Max Meisel. The Premier Publishing Co.
$7.50 6 x 9; xii + 749 *Brooklyn, N. Y.*

Mention has been made in a previous issue of THE QUARTERLY REVIEW OF BIOLOGY of the first two volumes of this highly useful work on American natural history. The present volume, prepared with the same painstaking thoroughness that characterized the preceding volumes, covers the last twenty years of the "pioneer century." In addition to the bibliographical studies of those institutions founded between 1845 and 1865, chronological tables, etc., the volume contains a bibliography of books and miscellaneous articles on natural history published separately or in serials of institutions not considered individually in the bibliography proper. An amazing amount of material, much of it from inaccessible sources, has been collected and skillfully arranged within the three volumes of this work. It is a valuable contribution to the history of the development of biological sciences in America.

MODERN BIOLOGY. *A Review of the Principal Phenomena of Animal Life in Relation to Modern Concepts and Theories.* *By J. T. Cunningham.* *E. P. Dutton and Co.* $3.00 5½ x 8½; xii + 244 *New York*

The author believes that modern genetics as it appears in the gene theory of T. H. Morgan, and the mechanistic principles of modern biologists generally do not explain many facts of biology. He cites what he considers are important objections, and belabors the authors of opposing views with great energy, though his own hormonic-vitalistic brand of Lamarckian eye-water has failed to attract any considerable body of followers, in spite of the fact that it has been in the field for a long time. Cunningham's writings are always interesting. The not negligible amount of valuable material they contain would probably gain more respectful attention and exert more influence if their whole viewpoint were somehow less wrong-headed and intolerant.

HOLIDAY POND.
By Edith M. Patch. *The Macmillan Co.*
$2.00 7½ x 8; 147 *New York*

We can imagine no more delightful way for a child to get its introduction to natural history than by following the adventures of Rana, the yelping frog, of Lotor, the raccoon, of Picta, the painted turtle, of Sandy, the swallow, and of the other creatures that Dr. Patch writes about.

GRUNDLINIEN DER EXPERIMENTELLEN PLANKTONFORSCHUNG.
Die Binnengewässer Band VI.
By Einar Naumann.
E. Schweizerbart'sche Verlagsbuchhandlung
10 marks (paper) *Stuttgart*
11.50 marks (cloth)
6¾ x 10; ix + 100

Another number in the series on *Binnengewässer* of which some have been previously noted in this REVIEW. It deals with laboratory methods for the cultivation and experimental investigation of plankton.

AMERICAN CHEMISTRY. *A Record of Achievement. The Basis for Future Progress. Second Edition.*
By Harrison Hale. *D. Van Nostrand Co.*
$2.50 5 x 7¾; ix + 255 *New York*

The second edition of a brief and elementary account of the part played by chemistry in the great American industries. Two chapters have some biological interest (Water and Sewage, Sanitation and Medicine; Food).

HUMAN BIOLOGY

IMMIGRANT FARMERS AND THEIR CHILDREN.
By Edmund deS. Brunner.
Doubleday, Doran and Company, Inc.
$2.75 *Garden City, N. Y.*
5¼ x 8½; xvii + 277

The economic, social, and spiritual status of the immigrant farmer and his children is here set forth in considerable detail. Since one-fourth of the foreign-born population in the United States is dwelling in rural areas, a survey of the general immigrant population can hardly be considered complete without such a study. We find nothing alarming in these pages. On the contrary, the fears of the Nordic-minded should be somewhat allayed, if such a thing is possible. Such subjects

as of what nationality are the immigrant farmers, where are they living, are they succeeding in their chosen occupations, are they tending to become standardized American citizens, with whom are they marrying, and can their children compete successfully with the native-born children are discussed. The second part of the report consists of studies by different individuals of four widely separated communities of immigrant farmers, each group presenting its own problems. Particularly interesting is the history of the colony known as Castle Hayne, where, more or less by chance, there collected "Hollanders, Poles, Italians, Danes, Hungarians, Russians, and all other misfits who happened along." That the bringing together of these diversified types caused the melting pot to boil with considerable vigor the history of this colony discloses. There are given in appendices data and tables on naturalization from a special census tabulation of foreign-born in 177 villages, on intermarriage, on distribution of the immigrant farmer by states, etc. There are also charts of intelligence tests of children, and maps of distribution of the foreign-born farmer. The book is well indexed.

MODERN SCIENTIFIC KNOWLEDGE OF NATURE, MAN, AND SOCIETY. *By Frederick A. Cleveland with Collaborators.* *The Ronald Press Company*
$4.50 5½ x 8½; xvii + 592. *New York*

The Ronald Press has evidently concluded that the potential bond salesman or junior executive needs something in his college curriculum wider in its outlook than accounting or principles of salesmanship. Hence this textbook for an orientation course, by the Professor of United States Citizenship of Boston University and fourteen collaborators, among whom we note the names of F. H. Giddings, H. H. Newman, H. M. Parshley, and Harlow Shapley. It is, of course, impossible to give a really adequate résumé of our knowledge of nature, man and society in 592 pages, but at least the student will get some idea from the book of what the various sciences are about, the concepts that they find useful in dealing with their segment of human experience, and their chief conclusions. There are copious references appended to the chapters, but we fear that if the student aspires to a high grade in his course in accounting he will have little time to explore the sacred jungles of the sciences.

In reading the book one cannot help being struck by the difference in the present state of the physical and biological sciences on the one hand, and of some of the social sciences on the other. Physics, chemistry and biology have by no means solved all their problems, but although they do not know just where they are going, they have a fairly good idea of how to get there. The pessimism of some physicists may be diagnosed, we believe, as a temporary *Bauchweh* caused by a plethora of as yet undigested facts. In the social disciplines the anthropologists and some, at least, of the economists and sociologists have attained to a method which promises to lead them somewhere, but most of the *Gelehrten* in jurisprudence and political science have, apparently, not yet heard the news that Thomas Aquinas is dead. If these disciplines are ever to find their way out of the mediaeval slough in which they are floundering, it will be, we believe, through such work as that of Malinowski on the juridical aspects of anthropology and as that promised by the programs of the institutes for legal research at Yale and Johns Hopkins.

BIBLICAL ANTHROPOLOGY *Compared with and Illustrated by the Folklore of Europe and the Customs of Primitive Peoples.* *By H. J. D. Astley.* *Oxford University Press* $4.50 5¼ x 8½; 262 *New York*

This scholarly and well written book covers somewhat the same ground as Sir James Frazer's *Folk-Lore in the Old Testament*, but in less detail. It consists of a series of essays, most of which had previously appeared in periodicals; they are now revised and enlarged to form a continuous study in anthropology as throwing light on many otherwise obscure passages in the Bible and on survivals among the Hebrews of primitive ideas, such as animism, totemism, magic, and tree- and pillar-worship. Thus the stoning of Achan for taking of the spoils of Jericho and the smiting of Uzzah for touching the ark of Yahweh are examples of tabu; the pig is really a totem animal; the two pillars, Jachin and Boaz, set up by Solomon before his temple find their counterpart in the sacred pillars of the Phoenician and Cretan temples. The motto of the book might be the following passage from Driver, which is quoted with approval:

> The general result of the archaeological and anthropological researches of the past half-century has been to take the Hebrews out of the isolated position which, as a nation, they seemed previously to hold. . . . Their beliefs . . . their social usages . . . their religious institutions can no longer be viewed, as was once possible, as differing in kind from those of other nations, and determined in every feature by a direct revelation from Heaven; all, it is now known, have substantial analogies among other peoples.

This is a shocking book for a Vicar of East and West Rudham and Honorary Canon of Norwich to write. If any stray Fundamentalist happens to read it, he may well offer a solemn thanksgiving to Yahweh that *his* Pastor knows nothing of anthropology, either biblical or profane.

RESEARCH RECOMMENDATIONS OF THE SECOND CONFERENCE ON PROBLEMS OF THE DEAF AND HARD OF HEARING, *Washington, D. C., February 1 and 2, 1929. Reprint and Circular Series No. 88. Issued under the Auspices of the Division of Anthropology and Psychology of the National Research Council.* *National Research Council* 50 cents *Washington* 6¾ x 9¾; iv + 53 (paper)

This report is the outcome of the work of the Committee on Tactual Interpretation of Oral Speech and Vocal Control of the Division of Anthropology and Psychology in the National Research Council presented to the Division May 1927. The early studies of the committee indicated that it was advisable to make a survey of the research needs and opportunities in the whole field of the care and training of the deaf. A committee was organized under the chairmanship of Dr. Knight Dunlap, and presented its results at the First Conference in January 1928. It was realized then that more information concerning basal problems pertaining to the deaf was needed and, accordingly, the National Research Council appointed a committee of six members under the chairmanship of Dr. Rudolph Pintner to study the problem further. The results were presented at a Second Conference February 1 and 2, 1929, and constitute this report.

The recommendations are categorized under the heads: Surveys; problems of sensory stimulation; educational problems; measures of capacity and achievement; problems of infancy and early childhood; problems of emotional and social adjustment; problems of causation and prevention; research personnel and provisions. For each of these a series of subdivisions lists the suggestions in detail.

THE PHYSICAL BASIS OF SOCIETY.
By Carl Kelsey. *D. Appleton and Co.*
$3.50 5 x 8; xxii + 526 *New York*

An entertaining book for the lay reader who is interested in the general aspects of the relationship of man to his natural environment. As reading matter for the student of sociology this book must be of great value. A first edition (published in 1916) which the author describes as being "little more than lecture notes" in which the reader "bumped along from fact to fact," has been entirely rewritten and very much enlarged. The material, which has been drawn from many widely different sources, is skilfully presented. There is no attempt to force theories upon the reader. Rather, the author holds to a clear statement of the essential facts which investigation has thus far revealed. In brief, it may be said that the book gives a comprehensive survey of society as it is controlled by nature and as it controls nature, of investigations concerning the evolution and inheritance of man and the problems of society which have developed because of man's progress. Of special interest are the chapters on "Sex and society" and "Race differences." Each chapter concludes with a list of books and papers referred to in the text, while there is a lengthy list of suggested readings at the end of the book, and subject as well as name and source indices.

URSPRUNG UND VERBREITUNG DES MENSCHENGESCHLECHTS. *Eine Neubegründung des Darwinismus auf Grund der Polwanderungen und im Anschluss an die Theorie Wegeners.*
By Franz Koch. *Gustav Fischer*
11 marks (paper) *Jena*
13 marks (bound)
6½ x 9½; vii + 174 + 15 plates

According to Dr. Koch, Wegener's theories of continental drift and wandering of the poles explain not only the distribution, but also the origin, of species. As the poles changed their position and the climate of a given area changed accordingly, the members of any species inhabiting that area were faced by three alternatives: (1) to migrate, (2) to adapt themselves to the changed conditions, (3) to become extinct.

Dr. Koch is suffering from a bad attack of patriotism. The whole treatment of human types finds its climax in a dithyramb on *Deutschlands Mission.* The Germans, it appears, wedged in between the "passive fatalism" of the Russians and the "hypocritical and cold hearted energy of the Anglo-Saxons, whose goal is the conquest of the world, and to whom one might say, alas!—Nietzsche's phrase 'the blond beast' applies almost uniquely," compel the unwilling admiration of the world by their science and technology, their philosophy and art, their poetry and music, but reap a harvest of aversion, envy and ingratitude. So long as the North pole keeps in its present position and the Germans breed from their Nordic elements, the cultural advancement of mankind will depend on them. "A cessation of their creative activity would mean the triumph of Russian dullness or of Anglo-Saxon mediocrity—both dismal prospects."

THE STORY OF RELIGIOUS CONTROVERSY.
By Joseph McCabe (Edited, with an Introduction by E. Haldeman-Julius).
The Stratford Co.
$5.00 6 x 9¼; xviii + 623 *New York*

Why do publishers think it good policy to print the extravagant puffs now customary on the jackets of their books? For our part, when we read that a book is "by far the most scholarly, the most readable and the most fascinating book on

religion since Sir James G. Frazer's 'Golden Bough,' " and that its author is "one of the three greatest scholars in the world today," we open the book in a more Missourian state of mind than we should if the blurb had contained fewer superlatives. Although Mr. McCabe's book scarcely deserves such hyperbolic praise, it is an interesting and well-written account of comparative religion and ethics and of the history of the Christian church. The standpoint is frankly controversial, but the author seems to be an honest fighter. It is well for us to be reminded now and then that organized religion, even the Christian religion, is not all brotherly love and good works.

THE JEWS IN THE UNITED STATES, 1927. *A Study of Their Number and Distribution.*
By Harry S. Linfield.
The American Jewish Committee
New York
Limited edition; free distribution
5¼ x 8; 111

A report of a survey, far more extensive than any hitherto attempted, on the number and distribution of Jews in the United States, made under the auspices of the American Jewish Committee. Not only were data collected on their distribution among the divisions of the country and the states, but also among the cities, towns, villages and rural areas. This is all given in the report in detail. The analysis of the material yields a number of interesting facts. We note that the Jews have now spread to every place of 25,000 or over and that there are residents of Jewish faith in nearly 10,000 cities, towns, villages and rural areas in the country. As regards density, the records show that the Jews form 11.11 per cent of the total population in cities of the class of 100,000 population or over, while in the class of cities of the size of 10,000 to 25,000 they constitute 3.11 per cent, and only 1.61 per cent in the class of 25,000 to 100,000 population.

ALMA MATER *or The Future of Oxford and Cambridge. To-day and To-morrow Series. By Julian Hall.* *E. P. Dutton and Co.*
$1.00 4⅛ x 6; 96 *New York*

In this number of the To-day and To-morrow series a newly fledged Oxonian tells what the English undergraduate is thinking. Most, it seems, refuse to believe that life is real, life is earnest, but there is a more serious minded minority who have faith in Mr. Wells' Open Conspiracy and who by the next generation will have leavened the whole lump. "The new education will be inspired by a conscious purpose, to which each step will contribute. It will fashion your outlook in accordance with a definite plan. The whole idea of the conspiracy takes rise from a set of historical, biological and sociological realizations; and at school these will be summarized for you as they exist up to date." The prospect seems almost as alluring as the Soviet régime or one of Mr. Ford's factories. We submit also that a sometime scholar of Balliol should know that the word *data* is a plural form.

THE STORY OF RELIGION *as Told in the Lives of Its Leaders.*
By Charles F. Potter.
Simon and Schuster, Inc.
$5.00 6¼ x 9¼; xx + 627 *New York*

More than two centuries ago Swift noted that "to enter the Palace of Learning at the *great Gate*, requires an Expence

of Time and Forms; therefore Men of much Haste and little Ceremony, are content to get in by the *Back-Door*." If this was true in the spacious Eighteenth Century, it is doubly true in these bustling days. Outlines of history, of science, of art, of most other mental disciplines, increase and multiply. In the field of religion we have already had Lewis Browne's excellent *This Believing World*. And now Dr. Charles Francis Potter gives us a history of religion in the form of vivacious and sometimes rather flippant sketches of the lives of Akhenaten, Moses, Zoroaster, Jeremiah, Buddha, Confucius, Jesus, Paul, Augustine, Patrick, Muhammad, Aquinas, Nanak, the founder of the Sikhs, Luther, Nicon, the Russian reformer, Wesley, and a group of American religious leaders ranging from Roger Williams to Mary Baker Eddy. In the chapter on Jesus we find no mention of the conclusion of Drews, Couchoud, J. M. Robertson and others that Jesus was a wholly mythical character. Dr. Potter's treatment is essentially Renan watered down and jazzed up.

THE ART OF LIFE. *From the Works of Havelock Ellis.*
Selected and arranged by Mrs. S. Herbert. *Houghton Mifflin Co.*
$2.00 4¾ x 7¼; 149 *Boston*

As reading matter, these "precious" selections, chosen from a group of the famous Englishman's writings, are tantalizing and unsatisfactory. In glancing through the book one continually has the desire to see each or at least many of the selections exhibited in their native background rather than grouped with numerous other selections. Perhaps, however, the chief rôle of the book is to give a general survey of the comprehensive mind of this wise observer of human thought and action. His philosophy is summed up in one sentence. "All the art of living lies in a fine mingling of letting go and holding in." Again and again, throughout the anthology, under the different headings "love," "morality," "religion," and "philosopy" we find this *motif* appearing and reappearing in one form or another.

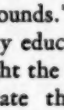
HIBERNIA *or The Future of Ireland. To-day and To-morrow Series.*
By Bolton C. Waller. *E. P. Dutton and Co.*
$1.00 4⅛ x 6; 96 *New York*

Mr. Waller points out that the two aspirations, Freedom and Unity, involved in the phrase "Ireland a Nation," are by no means necessarily connected, that there is much more likelihood of achieving unity under the present status of substantial self-government than under complete independence. His record of the accomplishments of the new régime in agriculture and hydro-electric development is encouraging. While solicitous for the preservation of an Irish national culture, he is sceptical of the program of the extreme Gaelicists. His remark on Protection is as germane on this side of the water as on the other: "To tax the general body of citizens in order to provide secure and comfortable incomes for inefficient manufacturers of unessential commodities is the worst possible national economy."

REPORT ON THE STATISTICAL ANALYSIS OF THE MEDICAL EXAMINATIONS (1924–25 TO 1926–27) OF STUDENTS OF THE MYSORE UNIVERSITY.
By K. B. Madhava. *University of Mysore*
6 x 9⅝; 80 (paper) *Mysore, India*

Measurements on 2,740 students at the two centers of Mysore University (Mysore

and Bangalore) covering a period of three years, constitute the basis of this report. In the group there are thirty-eight women. Records of body size and dimension, visual and other defects, diseases, etc., were made. In general the data of the two centers are analyzed separately. Also in some instances the Brahman and Non-Brahman records are segregated. One of the interesting results of such analysis is found in the comparison of body weight of the two sects, where there is indication that the "Non-Brahmans are as a rule heavier than their Brahman colleagues by above 4 to 7.5 pounds." Regarding the effect of university education in gain and loss in body weight the results, while not conclusive, indicate the importance of collecting extensive and reliable data on this point. The report closes with a brief historical account of medical inspection of students in various countries and cities.



AMONG THE FOREST DWARFS OF MALAYA.

By Paul Schebesta. Translated by Arthur Chambers. *Hutchinson and Co., Ltd.*

21 shillings $5\frac{7}{8}$ x 9; 288 *London*

An interesting account of a stay of nearly two years among the inland forest dwelling dwarfs of Malaya, generally called Semang, or Orang-Utan (meaning literally "forest dwellers)." The book is abundantly and well illustrated with photographs. It contains a wealth of ethnological information. The Semang are nomads, and extremely shy. To accomplish anything in the way of investigation of them it is necessary to live with them and move with them. In tropical forests with an excessive rainfall this is not the most comfortable enterprise conceivable for a European. The author notes that the Semang are dying out.

A great dearth of women is making itself felt in their ranks and the vices of the cultured races are gaining a hold on them, especially opium-smoking. Syphilis is, as far as I know, unknown among them. European civilisation is digging the grave of the dwarfs of Malaya as it presses further into the interior, thrusting the Malays before it and crowding the nomadic Semang into an area too confined for them where they are being economically smothered.

There is a good map, and an index.

ZYGOURIES. *A Prehistoric Settlement in the Valley of Cleonae. Published for the American School of Classical Studies at Athens.*

By Carl W. Blegen.

Harvard University Press

$15.00 *Cambridge*

9 x 12; xviii + 227 + 22 plates

A magnificiently produced monograph giving the results of excavations of a prehistoric site, dating from the Bronze Age. The prehistoric deposit could be clearly divided into three main layers, Early, Middle and Late Helladic. The finds are discussed under four main heads: Architecture; tombs; pottery; miscellaneous objects. The book is beautifully and extensively illustrated, the colored plates of pottery being especially fine.

THE PRIVATE CORRESPONDENCE OF NICOLO MACHIAVELLI.

By Orestes Ferrara.

The Johns Hopkins Press

$2.25 6 x 9; xii + 130 *Baltimore*

Started by a systematic campaign of defamation and misrepresentation during his life-time, there has grown up during the centuries a legend about Machiavelli, which recent research is showing to have been almost wholly at variance with reality. Doctor Ferrara, a distinguished student of Machiavelli, in this readable

book makes a further contribution towards dispelling this myth. In truth Machiavelli was a kindly, shy man of high intelligence who somewhat naïvely attempted to look at human behavior realistically. He died a disappointed and puzzled man. We recommend this book.

METANTHROPOS *or The Body of the Future. To-day and To-morrow Series.*
By Ronald Campbell MacFie.
E. P. Dutton and Co.
$1.00 4⅛ x 6; 96 *New York*

The author does not think that evolution will change the human body much in the future, except perhaps as regards the neopallium, and while there are signs of deterioration in the race, on the whole the future looks good. It is certain that matrimonial selection will produce a healthy race and science will find cures or prophylactics for all diseases. This thesis is set out as a running commentary on the views of biological and anthropological authorities. We hope that the writer has looked more carefully into the views he quoted than he has into the spelling of their authors' names. We find, among others, such aberrations as Thomas Haxley, Smith Elliot, J. A. Thompson, Millgan (the physicist), as well as the Piltsdown man.

EXCAVATIONS IN OLYNTHUS. *Part I. The Neolithic Settlement. The Johns Hopkins University Studies in Archaeology No. 6.*
By George E. Mylonas.
The Johns Hopkins Press
$7.50 7¾ x 10¾; xvii + 108 *Baltimore*

A detailed and beautifully illustrated account of the neolithic relics found in connection with the excavations at a site which has proved to be that of classical Olynthus, near a small village Myriophyto in Macedonia. The excavations have been made under the direction of Prof. David M. Robinson. The neolithic settlement flourished about 3000 B.C. Many of the finds are unique, including the first neolithic kiln to be discovered in Greek lands, the earliest zoomorphic vases, and the earliest religious idols so far discovered in Macedonia. Successive chapters deal with the site; architectural remains; pottery; figurines; celts; smaller finds; and affinities with other prehistoric sites. A valuable contribution to prehistory.

YOUR FAMILY TREE.
By David Starr Jordan and Sarah L. Kimball.
D. Appleton and Co.
$3.00 5 x 8; xi + 346 *New York*

This book is a pious monument to the fecundity of Lady Isabel de Vermandois and her two assistants, Robert de Beaumont, Count of Meullent, and William de Warren, Earl of Warren. All of us, or at least a lot of us, owe our existence here and now, according to the venerable and beloved author of this book, to Isabel's assiduity in what may be hoped—indeed believed—to have been a pleasant, and, for posterity, profitable task. Reginald, the Office Boy, has presented a difficult problem since this book fell into his clutches. He struts about the place like a pouter pigeon, because he says he is related to Aeneas, Wotan, and Calvit Coolidge, and can prove it by the book.

FOODS OF THE FOREIGN-BORN *in Relation to Health.*
By Bertha M. Wood. M. Barrows and Co.
$1.25 4¾ x 7½; ix + 110 *Boston*

This little book, a second edition, will be found extremely useful by dietitians and nurses working among our foreign populations. It will be of interest also to those who like to experiment with foreign cookery. Recipes of many palatable but fairly inexpensive dishes of different nationalities, largely European and Near East, are given. In an appendix there are analyses of the food values of the various recipes.

ACROSS THE GULF. *A Narration of a Short Journey through Parts of Yucatan with a Brief Account of the Ancient Maya Civilization.*

By Ralph F. Seymour.

The Alderbrink Press.

$7.00 7⅛ x 10⅛; 63 *Chicago*

A beautifully printed and illustrated book, issued in limited edition. It is a delightful account of a pleasure trip to Yucatan. The point of view is that of an artist, and the discussion of the Maya remains from that angle is illuminating. The woodcuts by the author are superb.

TRENDS IN PROTESTANT GIVING. *A Study of Church Finance in the United States.*

By Charles H. Fahs.

Institute of Social and Religious Research

75 cents 6 x 9; 67 (paper) *New York*

A study of the finances of the leading Protestant churches in the United States shows that the increase in gifts per capita failed to compensate for the decrease in value of the dollar during the war and in the case of gifts for congregational expenses through 1922. Since then they have more than compensated. The proportion of these gifts devoted to benevolences rose from 21 per cent in 1913 to 35 per cent in 1920 and then fell back to 22 per cent in 1927. The marked increase in indebtedness on church and parsonage property and the lessened appeal of the centralized efficiency systems installed after the war, as compared with the older, less organized, but more personal methods, are suggested as possible causes of this weariness in good works.

RACE CROSSING IN JAMAICA. *Carnegie Institution of Washington Publication No. 395 (Paper No. 36 of the Department of Genetics).*

By C. B. Davenport and Morris Steggerda (In collaboration with F. G. Benedict, Lawrence H. Snyder, Arnold Gesell, Inez Dunkelberger Steggerda and many residents of the colony of Jamaica).

Carnegie Institution of Washington

$7.00 (paper) *Washington*

$8.00 (cloth)

6¾ x 10; ix + 512

An anthropometric, physiological, psychological, developmental and eugenic study of Jamaican whites, blacks, and hybrids. The blacks showed themselves superior to the whites in tests of musical capacity, but inferior in operations which require good judgment. The greater likeness of the hybrids to the blacks than to the whites is explained as due to back crossing with the blacks rather than to dominance of the Negro characters.

PROCEEDINGS FIRST COLLOQUIUM ON PERSONALITY INVESTIGATION. *Held Under the Auspices of the American Psychiatric Association. Committee on Relations with the Social Sciences. December 1–2, 1928.* *Lord Baltimore Press*

60 cents (paper) 5¾ x 9⅛; 102 *Baltimore*

$1.00 (Cloth)

The record of a get-together meeting between a group of psychiatrists and a group of social scientists. The reader is left at the end wondering with little Peterkin what good came of it at last, whether either side attained a better grasp of the other's point of view than at the beginning.

DER NORDISCHE MENSCH. *Die Merkmale der nordischen Rasse mit besonderer Berücksichtigung der rassischen Verhältnisse Norwegens.*
By Halfdan Bryn. *J. F. Lehmanns Verlag*
9 marks (paper) *München*
11 marks (cloth) 6 x 9; 166

An excellent treatise on the physical characteristics and ethnology of Nordic man, with the Norwegian as a particular example treated in detail, written by one of the most distinguished Norwegian anthropologists. The book is attractively printed and abundantly illustrated. There is a bibliography of 112 titles, and a good index.

AGRICULTURAL EDUCATION IN THE UNITED STATES.
By Whitney H. Shepardson.
The Macmillan Co.
$1.50 $4\frac{7}{8}$ x $8\frac{3}{8}$; viii + 132 *New York*

Within its limitations of space this book is perhaps the best statement that has ever been made of the history, present status, opportunities and weaknesses of agricultural education and research in the United States. It was prepared as a report for the General Education Board, while the author was in the employ of that body, but we are told that now the General Education Board does not endorse its substance. The author was returned to business life "after a strange and stimulating interlude."

REPORT OF WAGE AND PERSONNEL SURVEY. *Field Survey Division, Personnel Classification Board. 70th Congress, 2d Session. House Document No. 602.*
U. S. Government Printing Office
60 cents $5\frac{3}{4}$ x $9\frac{1}{8}$; v + 511 *Washington*

A detailed study of the problem of the salaries and classification of government employees. Extensive data on salaries of comparable workers outside the government are presented in detail. The volume will be useful to the student of human biology for reference purposes.

HEREDITY AND PARENTHOOD.
By Samuel C. Schmucker. *The Macmillan Co.*
$2.50 $5\frac{1}{2}$ x 8; x + 322 *New York*

An interesting, well written book on the biological foundations of eugenics, intended for the reader without training in biology. It is one of the best books of the sort we have seen.

RACE AND POPULATION PROBLEMS.
By Hannibal G. Duncan.
Longmans, Green and Co.
$2.50 $5\frac{1}{4}$ x 8; xv + 424 *New York*

In this book the author "has endeavored to combine the old type of textbook with the more recent type of a book of readings, and has striven to refrain from injecting his own conclusions." If the result sounds rather like the *Literary Digest* it at least proves to the student that Doctors —of Philosophy as well as of Medicine— disagree.

VULCAN *or The Future of Labour. To-day and To-morrow Series.*
By Cecil Chisholm. *E. P. Dutton and Co.*
$1.00 $4\frac{1}{2}$ x 6; 95 *New York*

As a result of future improvements in management and machines the worker—

as soon as the slight detail of the utilization of atomic energy is solved—will, after his one day a week of labor, be able to step into his airplane for a six day jaunt on the continent. There is no indication in the book that Mr. Chisholm has ever heard of Malthus.

HEREDITY AND HUMAN PROBLEMS. *A Series of Eight Radio Talks. (With Select Bibliography). Radio Publication No. 50, University of Pittsburgh.*
By P. W. Whiting. University of Pittsburgh 60 cents $5\frac{3}{8}$ x $8\frac{3}{4}$; 87 (paper) *Pittsburgh*

In this group of necessarily popular lectures the author has succeeded in giving to the layman a very fair view of some of the present day problems of heredity and their importance in human relations.

ZOOLOGY

EOÖRNIS PTEROVELOX GOBIENSIS. *By Augustus C. Fotheringham.*
The Buighleigh Press
75 cents $7\frac{1}{2}$ x 10; 34 (paper) *London*
(Copies may be obtained from Dr. Lester W. Sharp, N. Y. State College of Agriculture, Cornell Univ., Ithaca, N. Y.)

This monograph in a first edition of 500 copies reports the results of the British Museum's expedition to the Gobi desert to secure definitive information regarding the creature designated by Linnaeus *Micropteron asiatica*, renamed by the authorities *Eoörnis pterovelox gobiensis* and popularly known as the "woofen poof." This rare bird was known to prehistory and antiquity, reproductions of it being found on the walls of the Crô Magnon caves, in an amulet of King Tut-ankh-amen and being mentioned by the Roman historian Eutropius as well as by Marco Polo. At present it is found only in a circumscribed region of the Gobi desert where the expedition accordingly went to make its observations.

The investigation covered the biology of the animal in a broad sense; general morphology, habits, life history, anatomy, embryology, physiology, pathology, psychology, taxonomy, phylogeny, and sociology are reviewed in separate sections. Some, if not all, of the findings are remarkable. We have not the space to present them adequately, but a few of the more startling may be mentioned.

The eggs, which have a flexible, rubber-like shell, hatch out twins, a male and a female comprising each batch. These same individuals later mate, the animal being monogamous, although careful cytological studies show that the eggs develop parthenogenetically. The food consists almost exclusively of the sand-flea *Fugifex pugens* which the bird consumes in large numbers, a contemporary substitute for the now extinct Crô Magnon worm. The flight is extremely rapid, reaching as high as 600 km. per hour, and the vibrations of the wings emit a characteristic note pitched usually at G below middle C. Of interest in this connection is the discovery that the albino variety is soundless, owing probably to an alternation in the use of each wing, one neutralizing the sound of the other. Psychological studies prove that *a priori* and *a posteriori* reasoning are equally potential. When placed exactly between two piles of sand-fleas, the bird languished to a death of starvation, unable to decide from which pile to feed. Ordinarily it dies from 7 to 35 years of age from deliquescence of the lumbar ganglion, resulting in a form of avian Bright's disease. The book is amply illustrated with photographs and diagrams, and written in a properly reserved but extremely entertaining style.

In view of the scholarly care with which the work is otherwise constructed we can excuse the typographical error in the spelling of the publishing house from which it was issued. Obviously it should read D'm Bigleigh Press.

CATALOGUE OF THE MADREPORARIAN CORALS IN THE BRITISH MUSEUM (NATURAL HISTORY). *Vol. VII. A Monograph of the Recent Meandroid Astræidæ.*

By George Matthai.

The British Museum (Natural History) *London*

£3, 3s. 9½ x 12¼; v + 288 + 72 plates

This magnificent monograph completes an investigation begun in 1914 by Prof. J. Stanley Gardiner.

An attempt is made at a revision of the Meandroid Astræid corals based on a comparative study of their hard and soft parts. But of the twenty-eight genera and sixty species described, the soft parts of only fourteen genera and twenty-three species were available for study. The structure of the polyps of eight genera and sixteen species is described for the first time. Three of the genera, viz. *Isophyllastræa* gen. nov., *Protomussa* gen. nov. and *Caulastræa* Dana, do not belong to the Meandroid group as they have distinct corallites. In addition to these, notes on *Tridacophyllia manicina* Dana, and on three doubtful species, viz. *Mussa lacinians* M. Ed. & H., *Favia astræiformis* (M. Ed. & H.) and *Favia spongiosa* (Dana) are also included.

The plates accompanying this Monograph contain photographs of some of Dana's types taken while I was working in the U. S. National Museum, Washington, of the existing types of Lamarck, Milne Edwards and Haime, Ehrenberg and Esper, and some of Rehberg's, as well as of a specimen believed to be Linnæus' type of *Madrepora areolata* in the collection of the Linnean Society of London. Six figures cited by Linnæus under *Madrepora labyrinthiformis, M. areolata* and *meandrites*, and one on which P. L. S. Müller based *Madrepora amaranthus* (1775), are also reproduced. The remainder of the photographs are of representative specimens in the British Museum (Natural History).

The heliotype plates are superb.

EXPERIMENTAL-ZOOLOGIE. VI BAND: ZOONOMIE. *Eine Zusammenfassung der durch Versuche ermittelten Gesetzmässigkeiten tierischer Formbildung (experimentelle, theoretische und literarische Übersicht bis einschliesslich 1928).*

By Hans Przibram. *Franz Deuticke*

40 marks *Vienna*

7 x 10¼; viii + 431 + 16 plates (paper)

Of this sixth volume of Prof. Przibram's review of the results of experimental zoology, 140 pages are devoted to bibliography. What would be text figures (line cuts) in normal book producing practice, are relegated to the back of the book as plates. The arrangement of the material is taxonomic in the major portion of the text. Altogether it seems to us that it would have been possible to make a more interesting and readable book than this is, considering the enormous amount of labor which went into its preparation.

FIELD BOOK OF MARINE FISHES OF THE ATLANTIC COAST FROM LABRADOR TO TEXAS. *Being a Short Description of Their Characteristics and Habits with Keys for Their Identification.*

By Charles M. Breder, Jr.

G. P. Putnam's Sons

$5.00 4 x 6¾; 332 *New York*

This excellent "handy volume," the first of its kind to appear, will be found extremely useful by both naturalist and angler. While the material has been culled largely from technical books and papers, the author has also contributed much new material from his own observations and wide experience. The book opens with brief introductory chapters on the identification, range, and manner of life of marine fishes. The descriptions of general habits, range and prominent features of each species are given in great

detail, while the identification of these is greatly facilitated by the excellent colored and black and white plates, as well as by the innumerable line drawings. At the end of the volume there is an all too brief bibliography, a glossary of technical terms, and an index.

A MANUAL OF EXTERNAL PARASITES.

By Henry E. Ewing. *Charles C. Thomas*
$4.50 *Springfield, Ill.*
5⅞ x 8¾; xiv + 225

This volume deals with the external anatomy, life history and natural relationships of the five major groups of ectoparasites: the mites, the ticks, the biting lice, the sucking lice, and the fleas. Keys are given to most of the known genera. There are numerous excellent illustrations, which will greatly assist in the identification of the different forms, and useful suggestions for their control. The book is well documented and will be found valuable as a reference book by those whose line of work requires a knowledge of parasitology, as well as by the student of general biology and entomology.

EXPERIMENTS IN MARKING YOUNG CHINOOK SALMON ON THE COLUMBIA RIVER, 1916 TO 1927. *Bureau of Fisheries Document No. 1047.*

By Willis H. Rich and Harlan B. Holmes.
U. S. Government Printing Office
40 cents *Washington*
7½ x 11; 215–264 + 21 plates (paper)

The results of these experiments, covering a period of eleven years, seem to give conclusive evidence of the "homing" instinct of chinook salmon. None of the tagged fish, liberated in the Columbia River and wandering as far afield as the waters of Southern Alaska, have been found in any other river stream than in the Columbia. Normally the fish tend predominately to return to spawn in the tributary in which they spent their early lives. The experiments indicate that the best plan in stocking waters is to plant each stream with eggs native to that stream. Other facts reported on in the paper are the "percentage of return," "success of long and short periods of rearing," "interpretation of scales," "time of entering fresh water," and "age at maturity." The text includes numerous tables concerning the results of the thirteen series of experiments carried on at different hatcheries on the Columbia River. Photographic plates show the system of scale markings, etc. while there is a bibliography of sixteen titles.

MANUAL OF THE VERTEBRATE ANIMALS OF THE NORTHEASTERN UNITED STATES INCLUSIVE OF MARINE SPECIES. *Thirteenth Edition.*

By David Starr Jordan (with an Introduction by Barton W. Evermann).
World Book Company
$4.00 5⅜ x 9; xxxi + 446 *Yonkers, N. Y.*

A revised edition of the vertebrate manual which has been standard for fifty years. The district covered is, approximately, the northeastern United States and southern Canada, extending from Labrador westward to and including Manitoba and North Dakota, and southward to and including North Carolina and Kansas. The resident shore fishes of the region are included, but for the most part not the strays from the Gulf Stream or the fishes of the Deep Seas. The birds which are chance wanderers from Europe or the West Indies are also omitted or, at the most, merely mentioned.

THE PLANT LICE OR APHIDAE OF GREAT BRITAIN. *Vol. III.*
By Fred V. Theobald. *Headley Brothers*
30 shillings *London*
5½ x 9; vi + 364 (paper)

With the issuance of this volume the description of the British aphides, so far as at present known, is completed. The investigator has maintained the same standard of excellence exhibited by the previous volumes, both of which have been mentioned earlier in these columns. The third section opens with the subtribe Chaitophorina. Students of agriculture, horticulture and forestry will find this completed work an invaluable addition to their list of reference books.

OUTLINES OF GENERAL ZOÖLOGY.
By Horatio H. Newman. *The Macmillan Co.*
$3.50 5½ x 8½; xxii + 541 *New York*

A revised second edition of this admirable text. The changes from the first are considerable. Material has been added to make it adequate for a year course, and the last part on general principles of zoölogy redone under the title "Dynamic Aspects of Zoölogy," giving it more of a physiological content than it had in the former arrangement.

COREGONID FISHES OF THE GREAT LAKES. *Bureau of Fisheries Document No. 1048.*
By Walter Koelz.
U. S. Government Printing Office
$1.10 7¼ x 11; 346 (paper) *Washington*

This excellent monograph contains the report of an extensive study of the coregonid fishes from an economic as well as a scientific viewpoint. The author deals with the distribution of these forms in the various waters of the Great Lakes region, their natural history and the conditions under which they live, their origin and evolution, and their relationships with one another and with the coregonids of other parts of America, as well as Asia and Europe. The paper contains 101 tables, a number of figures, a lengthy bibliography and distribution maps of the various lakes studied.

CAPITA ZOOLOGICA. II. *afl. 7. Freilebende Marine Nematoden von der Nordwest-Kueste Frankreichs (Trébeurden: Côtes du Nord).*
By Hans A. Kreis. *Martinus Nijhoff*
10 guilder *The Hague*
9⅞ x 13; 98 + 8 plates (paper)

This monograph on the free-living marine nematodes of the coast of Brittany includes interesting sections on morphology and biology and ecology. The collection was rather rich. It included 485 individuals included in 34 genera with 46 species. It is well illustrated, indexed, and documented. It does credit to the series in which it is included.

TYPICAL FLIES. *A Photographic Atlas of Diptera. Series III.*
By E. K. Pearce.
Cambridge University Press
10 shillings net *London*
7 x 10⅛; xiv + 64

This little volume, the third in the series, presents us with a group of remarkably fine photographs of many flies of diverse groups of British Diptera. The usefulness of this volume to the student is enhanced by the brief notes on size, coloring, general appearance and habits accompanying each illustration, the preface giving full information for collecting and preserving specimens, a bibliography, and an index.

LABORATORY GUIDE AND REVIEW MANUAL FOR GENERAL ZOÖLOGY.
By H. H. Newman. *The Macmillan Co.*
$1.10 5½ x 8½; ix + 87 *New York*

An excellent laboratory guide designed especially to accompany the author's textbook in general zoology. There is an appendix containing a set of review questions on parts of the text not illustrated by laboratory studies. Teachers will find this a most satisfactory book to put in the hands of their students.

DIE TIERWELT DER NORD- UND OST-SEE. *Lieferung XIV.*
Edited by G. Grimpe and E. Wagler.
Akademische Verlagsgesellschaft m.b.H.
15.60 marks 6 x 9; 188 (paper) *Leipzig*

This number of the handbook on the fauna of the North and Baltic Seas, of which earlier parts have been noticed in these columns, is by K. Stephensen, and treats of the Amphipoda.

ANIMAL BIOLOGY.
By Henry H. Lane.
P. Blakiston's Son and Co.
$3.50 5½ x 8½; 588 *Philadelphia*

An introductory zoology text for college students.

BOTANY

THE RÔLE OF RESEARCH IN THE DEVELOPMENT OF FORESTRY IN NORTH AMERICA.
By I. W. Bailey and H. A. Spoehr.
The Macmillan Co.
$1.50 5¼ x 7¾; xiii + 118 *New York*

"In a paper presented before the National Academy of Sciences in 1924, Colonel W. B. Greeley, then Chief of the United States Forest Service, outlined the gravity of the situation as to present and future national timber resources in proportion to obvious needs, and asked the counsel of the Academy in meeting the situation. Following Colonel Greeley's request, the National Academy appointed a representative group to inquire further into the facts and to report its findings. The publication of the report in this volume presents in some measure the answer of the Academy through its committee to the questions proposed by the Chief of the Forest Service."

It is a carefully considered inquiry into the present opportunities and future possibilities of research as an aid to silviculture, based on the expert knowledge of the authors and a painstaking special investigation made for the purposes of this report. The chapter headings will indicate its scope: Agriculture as contrasted with silviculture; Research and its applications in silviculture as contrasted with agriculture and medicine; present status of the forestry movement in the United States; existing agencies for descriptive and empirical experimental investigation in forestry; Can research in the basic experimental aspects of forestry be developed and handled adequately by existing agencies?; New agencies required for research, particularly in the fundamental physiological and ecological aspects of forestry.

PLANT ECOLOGY.
By John E. Weaver and Frederic E. Clements.
McGraw-Hill Book Co., Inc.
$5.00 5¾ x 9; xx + 520 *New York*

This comprehensive treatise, the outgrowth of years of research and teaching by the authors, furnishes teacher and student alike with a valuable source book

and practical guide. The book contains a great wealth of material for laboratory and field work, so that the teacher can easily adapt his course to the particular conditions under which he works. All of the experimental work for greenhouse and laboratory has been thoroughly tested over a long period of time. The studies for field work are developed on broad general lines but contain many suggestive approaches to the more detailed work for special regions. Numerous excellent illustrations, a list of over 600 titles, and a detailed index, all add to the usefulness of this volume.

BIBLIOTHECA BOTANICA. *Heft 97. Untersuchungen über die Abhängigkeit des pflänzlichen Wachstumsverlaufes und der erreichten Endlänge von konstanten Temperaturgraden, by Karl Silberschmidt.*

E. Schweizerbart'sche Verlagsbuchhandlung

36 marks 9 x 12¼; 95 (paper) *Stuttgart*

A thorough, monographic account of an experimental study of seedling growth. Peas and oats were the forms chiefly used, and the analysis concerns mainly the influence of temperature, light, and moisture. The results of this detailed research are too voluminous to review here. Every plant pathologist will wish to consult the original.

BOTANY. *A Textbook for College and University Students.*

By William J. Robbins and Harold W. Rickett

D. Van Nostrand Co., Inc.

$3.75 6 x 9; xxiii + 535 *New York*

Designed to present more of fundamental biological principles than the usual text, so as to meet the needs of the student for whom an elementary course in botany is the only formal work in biological science he will ever do.

LABORATORY MANUAL OF BOTANY.

By Heber W. Youngken.

P. Blakiston's Son and Co., Inc.

90 cents *Philadelphia*

8 x 10½; vi + 73 (paper)

Designed to accompany the author's textbook, *Pharmaceutical Botany*, for students pursuing laboratory work in pharmaceutical institutions.

A COLLEGE TEXTBOOK OF BOTANY FOR FIRST YEAR STUDENTS.

By David M. Mottier.

P. Blakiston's Son and Co.

$3.50 5½ x 8¼; 516 *Philadelphia*

An elementary text.

MORPHOLOGY

DENTAL ANATOMY. *A Graphic Presentation of Tooth Forms with an Original Technique for Their Reproduction.*

By M. Diamond. *The Macmillan Co.*

$5.00 5½ x 8½; xvi + 300 *New York*

A detailed and extensively illustrated descriptive anatomy of the teeth of man leading up to an elaborate but clearly described technique of reproducing by carving, symmetrical tooth forms. While it will be chiefly useful for dentists, it is a book which should be in every anatomical library. The descriptive portions have been done with great thoroughness, but one misses anything in the way of bibliographical references.

THE EARLY EMBRYOLOGY OF THE CHICK. *Third Edition.*

By Bradley M. Patten.

P. Blakiston's Son and Co.

$2.50 6 x 9; xiii + 228 *Philadelphia*

A new edition of the text which is the pedagogical standard for introducing the American student to the science of embryology.

PHYSIOLOGY AND PATHOLOGY

THE ANNALS OF THE PICKETT-THOMSON RESEARCH LABORATORY. *Volume IV, Part II. The Pathogenic Streptococci. An Historical Survey of Their Rôle in Human and Animal Disease.* *The Williams & Wilkins Co.* *Baltimore*

$10 per volume (Parts I and II)

8¾ x 11¼; viii + 243 (paper)

The issue, containing the second group of studies on pathogenic streptococci, completes Volume IV of the Annals from the Pickett-Thomson Research Laboratory. Reviews of two previous volumes on streptococci have appeared earlier in these columns. In the present number the first two monographs (V and VI in the completed volume) on "the rôle of the streptococci in acute and subacute suppurative arthritis" and "the rôle of the streptococci in chronic arthritis," are by the co-workers David and Robert Thomson. The third and last monograph (VII) is by H. Warren Crowe on "Rosenow's hypothesis of elective localization." It has been the aim of these workers to bring together a more or less complete résumé of researches in this field, as well as to present important views and theories held by various clinicians. The present work includes also reports on a study of "the experimental production of foci of infection in rabbits and its bearing on osteo-arthritis," by H. Warren Crowe and "a simple and rapid test for the differentiation of streptococci," by David Thomson. Each monograph includes extensive reference lists. There are excellent photographic plates belonging to Monograph VI. Detailed author and subject indices for Volume IV complete the work. Volume V, to be issued at a later date, will contain studies on the rôle of the streptococci in oral and dental sepsis and tonsillitis.

THE GENUINE WORKS OF HIPPOCRATES. *Translated from the Greek with a Preliminary Discourse and Annotations. Two volumes in one.*

By Francis Adams. *William Wood and Co.*

$6.00 *New York*

6 x 9⅛; Vol. I, vi + 390
II, 366

It is now eighty years since the good physician of Banchory published his translation of the Hippocratic *corpus*, of which this is a new edition. The translation is excellent, but progress in medical knowledge has rendered obsolete many passages in the introduction and notes.

The astonishing modernity of the ancient Greeks is strikingly shown in the passage on the Macrocephali in the *Airs, Waters and Places*:

> There is no other race of men which have heads in the least resembling theirs. At first, usage was the principal cause of the length of their head, but now nature coöperates with usage. They think those the most noble who have the longest heads. It is thus with regard to the usage: immediately after the child is born, and while its head is still tender, they fashion it with their hands, and constrain it to assume a lengthened shape by applying bandages and other suitable contrivances whereby the spherical form of the head is destroyed, and it is made to increase in length. Thus, at first, usage operated, so that this constitution was the result of force: but, in the course of time, it was formed naturally; so that usage had nothing to do with it; for the semen comes from all parts of the body, sound from the sound parts, and unhealthy from the unhealthy parts. If, then,

children with bald heads are born to parents with bald heads; and children with blue eyes to parents who have blue eyes; and if the children of parents having distorted eyes squint also for the most part; and if the same may be said of other forms of the body, what is to prevent it from happening that a child with a long head should be produced by a parent having a long head?

If this is not an anticipation both of Lamarckianism and of Darwin's hypothesis of pangenesis we are ready to eat our hat.

MEDICINE. *Its Contribution to Civilization. By Edward B. Vedder.*

The Williams & Wilkins Co.

$5.00 6¼ x 9½; ix + 398 *Baltimore*

A well written account for the general reader of the present state of medicine and especially of preventive medicine. Colonel Vedder begins with the recognition that in many diseases, such as tuberculosis and meningitis, not all persons exposed to the causative organism contract the disease, that both constitutional diathesis and environmental factors are predisposing causes. He then treats the infectious diseases, nutrition and nutritional diseases, diseases of the glands of internal secretion, degenerative diseases, the conquest of disease, the cancer problem, and modern preventive medicine. Unlike Hoffman he concludes that the increase in the published crude death rates from cancer is due to improved diagnosis and to changes in the age distribution of the population.

DEVILS, DRUGS, AND DOCTORS. *The Story of the Science of Healing from Medicine-Man to Doctor.*

By Howard W. Haggard. Harper and Bros.

$5.00 6 x 9½; xxii + 405 *New York*

The great advances that have occurred in medical practice are here thrown into bold relief by a method of hyperbolic contrast. The past is represented by filth, ignorance, bigotry, cruelty and suffering, relieved by an occasional generous and progressive individual. The present, except for the osteopaths *et al.*, is enlightenment and sanitation. This black and white picture of medical history fails to give a well balanced view of the gradual growth of medical knowledge and the important place that general science has had in it, but what it lacks of even objectivity it gains in high dramatic interest. The matters dealt with are chosen for their great human interest, childbirth, anesthesia, surgery, plague, etc., and the individual events for their melodrama and picturesqueness. The whole makes an exciting story, and it is superbly illustrated by reproductions from contemporary artists and cartoonists. It is well done as a piece of bookmaking and contains an index.

THE MINIMUM PROTEIN REQUIREMENTS OF CATTLE. *Report of Committee on Animal Nutrition. Bulletin of the National Research Council No. 67.*

By H. H. Mitchell.

National Research Council

$1.00 6¾ x 9¾; 84 (paper) *Washington*

This report gives the results of applying a method of measuring protein requirements, proposed by the Sub-Committee on Animal Nutrition, to experimental data on beef and dairy cattle already available. Concerning the results obtained, the author states that they are

not to be compared with values already current in the literature. Heretofore, protein requirements have been expressed in two ways, either in terms of digestible crude protein or in terms of digestible true protein. Statements thus expressed are ambiguous, since they are not related to protein of any definite or known biological value. In this report, protein

requirements are expressed in terms of animal expenditures or storages of nitrogen, converted for convenience into conventional protein (N × 6.25). They may be considered as representing the requirements for digestible crude protein possessing a biological value of 100. They are thus definite in their significance, minimal in the truest sense of the word, and adaptable to any protein mixture the biological value of which may be satisfactorily estimated.

There are 32 tables in the text and a bibliography of 99 titles.

REPORTS SUBMITTED BY THE RADIOLOGICAL SUB-COMMISSION. *League of Nations, Health Organisation, Cancer Commission. Publication III. Health 1929. III. 5. Official No. C. H. 788.*

League of Nations

$1.50 7½ x 10; 82 (paper) *Geneva*

Contains the rules adopted for obtaining full and comparable statistics in regard to incidence and treatment of utero-vaginal carcinoma and several special reports of particular experience with radiotherapy as follows: Report on the technique and results of the treatment of cancer of the uterine cervix at the Radiumhemmet at Stockholm, by J. Heyman; Report on the technique and results of the radiotherapeutic treatment of cancer of the uterine cervix at the Radiological Section of the University Gynaecological Clinic at Munich, by F. Voltz; Report on radiotherapy in the treatment of epitheliomata of the uterine cervix at the Radium Institute at Paris, by A. Lacassagne.

THE HUMAN MECHANISM. *Its Physiology and Hygiene and the Sanitation of its Surroundings.*

By Theodore Hough and William T. Sedgwick. Second revised edition by J. A. Waddell.

Ginn and Co.

$3.00 5 x 7½; 691 *Boston*

A text written for the very ignorant public and saturated with the noble spirit of uplift. Tobacco, it is stated somewhat elaborately, is taken in three forms: (1) smoking, (2) snuffing, (3) chewing. All are bad to different degrees and lead to

(1) disorders of the digestive system or capricious appetite frequently needing coaxing by tonics; (2) enlargement of the heart and irregularity in its action, called tobacco heart, in which changes in position and emotions produce a violent increase in rate; and (3) impairment in nervous activity. . . . It can hardly be doubted that many a young man has failed to make the most out of life because this habit contracted in youth has brought about a weakening of the foundations upon which he subsequently had to build, and, in consequence, he became stunted mentally and physically.

Some states, it is mentioned suggestively, prohibit its sale *by law*.

COMMON COLDS. *Causes and Preventive Measures.*

By Leonard Hill and Mark Clement.

William Heinemann, Ltd.

7 s. 6 d. net *London*

5⅜ x 8⅜; 126

An excellent treatise on common colds, giving not only a great deal of information about the conditions under which the cold germ flourishes and the more serious disabilities which so frequently follow an infection, but some very sound advice on the prevention of colds, particularly as regards diet, exercise, clothes, and rest. In brief, the warning is that he who would protect himself from colds must guard against loss of tone in his protective structure. If these structures—the skin (which is rated first in importance) and the mucous membranes of nose and throat—have the ability to react quickly to changes in temperature and humidity, then the ever present cold germ has little opportunity to get in its devastating work.

ARE THE GREAT AMERICAN SALT, SUGAR, WHITE FLOUR, HOT DRINKS, VINEGAR, HEN EGG AND TOBACCO DEBAUCHES PARTIALLY RESPONSIBLE FOR CANCER, INFANTILE PARALYSIS, INFLUENZA, CONSUMPTION, WHOOPING COUGH AND THE ALARMING INCREASE IN HEART FAILURE?

By H. H. Harwood. *H. H. Harwood*
For price inquire of author. *Richmond, Va.*
6 x 9; 208 (paper)

This curious volume consists very largely of quotations from the writings of well-known investigators, physicians, surgeons, health officers, and uplifters, interspersed with the author's loosely drawn conclusions that certain articles of diet and tobacco are in part responsible for certain diseases. We offer for the reader's consideration a sample of the result of his mental gyrations: "So, in studying Infantile Paralysis, only a one-tenth-educated physician could imaginably leave tobacco-smoke off his list of probably contributing causes."

MORPHEUS *or The Future of Sleep. To-day and To-morrow Series.*
By D. F. Fraser-Harris.
E. P. Dutton and Co.
$1.00 4⅛ x 6; 94 *New York*

Four-fifths of this essay deals with the "today" and one-fifth with the "to-morrow" of sleep. The first reviews the little that is known about its physiology and gives some common sense advice regarding its hygiene. The chapter on dreams could be a great deal more interesting than it is. About the only suggestion for improving sleep in the future is that anyone shouting, singing, or whistling after eleven P.M. be subject to arrest. Nothing is advanced respecting yowling cats or crying babies.

WORK OF THE CANCER COMMISSION. *League of Nations Health Organisation. (Exposé by Sir George Buchanan to the Health Committee. Extract for the provisional minutes C. H. /14th Session/P. V. 7, May 7th, 1929). Official No. C. H. 800.*
League of Nations
Not on sale *Geneva*
8 x 13; 8 mimeographed pages (paper)

Minutes of the oral report of the President of the Cancer Commission of the Health Section of the League of Nations regarding its work. The chief accomplishment of the Commission is the report on Radiotherapy noticed elsewhere in this number.

DIE HORMONE. *Ihre Physiologie und Pharmakologie. Vol. 1. Keimdrüsen, Hypophyse, Nebennieren.*
By Paul Trendelenburg. *Julius Springer*
28 marks (paper) *Berlin*
29.60 marks (cloth)
6¼ x 9½; xi + 351

The first of a two volume work reviewing the literature of the hormones of the gonads, the hypophysis and the suprarenal. It is very thoroughly done, and carries the references up to the end of 1928. There is a detailed subject index. The book is a useful contribution to the reference literature of a lively field of biology.

ESSENTIALS OF GENERAL PHYSIOLOGY.
By Eric Ponder. *Longmans, Green and Co.*
$3.60 5½ x 8; vii + 497 *New York*

An introductory text covering the ground broken by Bayliss' classic, *Principles of General Physiology*. It does not exhibit anything like the originality or fine literary quality of the latter.

YOUTHFUL OLD AGE. *How to Keep Young.*
By Walter M. Gallichan.
The Macmillan Co.
$2.50 5¼ x 7¾; xii + 236 *New York*

Pleasant, if rambling, discourse about personal hygiene in relation to the problems presented by senescence. The author says that constipation is the disease that ages. Meat he is opposed to.

BODILY CHANGES IN PAIN, HUNGER, FEAR AND RAGE. *An Account of Recent Researches into the Function of Emotional Excitement.*
By Walter B. Cannon. D. Appleton and Co.
$3.00 5¼ x 8; xvi + 404 *New York*

A second edition, which has been thoroughly revised and enlarged to include the results of all of the latest researches in bodily response to emotion.

SUNRAYS AND HEALTH.
By Ronald Millar. In collaboration with E. E. Free. Robert M. McBride and Co.
$1.50 5 x 7½; vi + 125 *New York*

A sound little book, simply and interestingly written for the general public, on the nature and biological properties of ultra-violet light.

BIOCHEMISTRY

DIE FERMENTE UND IHRE WIRKUNGEN. *Vierter Band: Die Technologie der Fermente.*
By Carl Oppenheimer. Georg Thieme
75 marks (paper) *Leipzig*
83 marks (cloth)
7¾ x 10¼; xvi + 370

Previous issues in the series which comprise this treatise on ferments have been noticed in these columns. The present volume constitutes Band IV of the general work and is devoted to the application of enzyme chemistry to industry. It is divided into two main divisions: the first of these is devoted exclusively to fermentation industries and is by Dr. Albert Hesse; the second considers different industries in successive chapters as follows: Ferments in the fat industry, by Dr. Emil Hoyer; Ferments in the milk industry, by Dr. Walter Grimmer; Ferments in the leather industry, by Dr. Otto Gerngross; Ferments in the gelatin and lime industry, by Dr. Otto Gerngross; Ferments of the pharmaceutical industry, by Dr. Peter Bergell and Herbert Carls; Ferments in the malt-extract industry, by Dr. Albert Hesse; Ferments in the textile industry, by Dr. Albert Hesse; and Ferments in the food industry, by Dr. Albert Hesse. A subject index is appended.

ELEKTRIZITÄT UND EIWEISSE, *insbesondere des Zellplasmas.*
By Hans Pfeiffer. Theodor Steinkopff
10 marks (paper) *Dresden*
10.50 marks (bound)
6 x 8¾; xii + 149

The series of which this book is a number attempts to give succinct reviews of the recent work in various branches of scientific research. The period covered is about the last fifteen years, but earlier work is included where it is germane to contemporary problems. The present volume covers the colloid and electrical properties of proteins and leads into a consideration of the play of these in protoplasm. It is well supplied with bibliography and contains an author and subject index that adds greatly to its value as a working reference.

PRACTICAL PLANT BIOCHEMISTRY. *By Muriel W. Onslow.* *Cambridge University Press*
12s 6d. 6¼ x 9¼; 206 *London*
$4.25 (*The Macmillan Co., New York*)

A third edition of this highly successful text. The chapter headings will indicate its scope: Introduction; the colloidal state; plant enzymes; chlorophyll; carbohydrates; the vegetable acids; fats and allied substances; aromatic compounds; proteins and amino-acids; glucosides; plant bases. There are bibliographical references appended to each chapter and a general index at the end.

ELEKTROCHEMIE DER KOLLOIDE. *By Wolfgang Pauli and Emerich Valkó.* *Julius Springer*
66 marks (paper) *Vienna*
68 marks (bound)
7 x 10¼; xii + 647

A substantial contribution to the literature of colloids. The foundation of the discussion is laid in an introductory part, reviewing the modern electrolytic theory of solution, and then going on to general and special colloid chemistry. The bibliographic documentation is extensive, and is brought up to date. The book is well indexed and constitutes a valuable reference work.

A CLASSBOOK OF PRACTICAL CHEMISTRY. *First Year.* *By J. Morris.* *Methuen and Co., Ltd.*
2 shillings 4¼ x 6¾; viii + 103 *London*

An excellent laboratory guide for a beginner's course in chemistry. While the author has designed it to accompany his *Introduction to Chemistry*, it is so arranged that it can be used in combination with any standard textbook for beginners. The unique method has been followed of placing on the left hand page the detailed directions for experiments, and on the opposite page the results and conclusions drawn from these experiments. It is intended that the right hand page shall be covered until the student has worked out and written up in his notebook his observations. He then can refer to the correct solution and discover his mistakes, if there are any. Naturally this method works best with those students who are not seeking easy methods to slide through a course.

PRÉCIS DE PHYSICO-CHIMIE BIOLOGIQUE ET MÉDICALE. *By André Dognon.* *Masson et Cie*
36 francs 5 x 7½ inches; viii + 310 *Paris*

This is a condensed summary of the principal data of physical chemistry which are of direct applicability to medicine. The general topics under which the material is divided are: Water and solutions; osmotic pressure; electrolytic ionisation; hydrogen ions; surface phenomena; viscosity; colloidal solutions; general properties of membranes. There is no index, or bibliography. The book is essentially of the quiz-compend type.

HERMES *or The Future of Chemistry. To-day and To-morrow Series.* *By T. W. Jones.* *E. P. Dutton and Co.*
$1.00 4⅛ x 6; 88 *New York*

In a single chapter unrelieved by humor or imagination the author dilates on the application of chemistry to practical problems in the future. Almost a half concerns fuel; the remainder various other subjects, including food and medicine. The author suggests little more than further developments of what chemistry is already doing.

SHORT LABORATORY COURSE IN GENERAL CHEMISTRY.
By Raymond E. Neal. *The Macmillan Co.*
$1.50 4⅞ x 7⅜; viii + 106 *New York*

Based on Bartlett and Ink's *The Principles of Chemistry and Their Application.* Enough material is provided to fill a two-hour period each week for sixteen weeks.

SEX

THE SEXUAL LIFE OF SAVAGES IN NORTH-WESTERN MELANESIA. *An Ethnographic Account of Courtship, Marriage and Family Life among the Natives of the Trobriand Islands, British New Guinea. 2 Vols.*
By Bronislaw Malinowski.
Horace Liveright
$10.00 for two volumes. *New York*
5¾ x 8⅝; xxviii + 279 (Vol. I)
viii + 322 (Vol. II)

In these two beautifully printed and illustrated volumes Professor Malinowski has made a contribution to ethnography of the very first importance. Here one aspect or division of his studies on the Trobriand Island people is completely worked up, in full and satisfying monographic form. The plan and the scope of the treatment are indicated by the chapter heads: The relation between the sexes in tribal life; the status of woman in native society; prenuptial intercourse; the avenues to marriage; marriage; divorce and the dissolution of marriage by death; procreation and pregnancy in native belief and custom; pregnancy and childbirth; customary forms of license; love-making and the psychology of erotic life; the magic of love and beauty; erotic dreams and fantasies; morals and manners; a savage myth of incest.

The thing which perhaps is of greatest general biological interest regarding the Trobriands is their ignorance of physiological paternity, a theme which Professor Malinowski has discussed in earlier works. Here he has developed it further with a wealth of detailed evidence. In doing this he uncovers a number of extremely puzzling observations regarding points in the physiology of human reproduction, the explanation of which is by no means clear.

Altogether the book is a notable one. We heartily agree with the statement made by Havelock Ellis in the preface:

> These phenomena he views with a characteristically wide outlook; while not neglecting the precise technique of the erotic art among the Trobriand Islanders, he duly investigates their whole sexual life in its aesthetic, emotional, family, and social implications. Now that he has shown the way, other students doubtless will be inspired to follow. But in this field not all who are called are chosen. The special combination of needed qualifications can rarely be found, and meanwhile the opportunities are every year diminishing. It may safely be said that *The Sexual Life of Savages in North-western Melanesia* will become a classic of which the value must increase with the passage of time.

HAVELOCK ELLIS. *Philosopher of Love.* *By Houston Peterson.* *Houghton Mifflin Co.*
$4.50 5½ x 8½; ix + 432 *Boston*

In *Havelock Ellis: A Bibliographical and Critical Survey* Isaac Goldberg gave us a pioneer study of one of the noblest and most significant figures of our age. In the present book Mr. Peterson, with the aid of unpublished documents and the co-operation of Ellis himself, paints a full-length portrait of his hero. We follow his development from the earnest boy, half comical, half pathetic, reared in the best traditions of English evangelicism, and aspiring to serve his God and his fellow men in the cure of souls, through the period of adolescent doubt in which he

sloughed off the orthodox beliefs of his boyhood, through his conversion, under the influence of James Hinton, to a vision of the universe as beauty, through his resolve to devote himself to making clear the problems of sex, a resolve followed *ohne Hast, ohne Rast* for more than thirty years through difficulties and obloquy which few men would have been willing to endure and bearing fruit in the classic series of *Studies in the Psychology of Sex*, and through his work as lay philosopher and interpreter of the art of life, to a ripe and serene old age. We do honor to Byrd and his fellow-explorers for facing hardships and perils in the quest of new geographical knowledge; it is meet and right that we should do greater honor to an explorer who has faced the suspicion of the compact majority and the persecution of narrow-minded officialdom in the quest of knowledge which would relieve some of the most poignant miseries from which mankind suffers.

Peterson makes it plain that Ellis was interested from the first in normal as well as abnormal sex psychology, and that it was largely owing to John Addington Symonds that the first volume of the *Studies* dealt with sexual inversion rather than with some other phase of the sex problem.

THE INTELLIGENT MAN'S GUIDE TO MARRIAGE AND CELIBACY.
By Juanita Tanner. *The Bobbs-Merrill Co.*
$3.50 6⅛ x 9¼; xvii + 312 *Indianapolis*

The author of this book, writing under an assumed name, conceives herself to be the daughter of John Tanner, of *Man and Superman* fame, and grand-daughter of the author of that play. Her attempt, apparently, is to produce a counterpart to Shaw's guide for the intelligent woman in the field of socialism and capitalism. One cannot but believe, however, that the really intelligent reader will lose patience before he has gone very far in these pages. To be sure, there is a certain amount of cleverness, but this soon wearies one. "Miss Tanner" has loosely thrown together a vast amount of material without making any clear cut development of the subject or any satisfactory presentation of facts. One reaches the conclusion that she probably favors "individual freedom" in the form either of a very loose family attachment or of celibacy since she says that

> "All the things that are real fun, like travel and physical danger and rash utterances, demand a singleness of mind; they can not be complicated by any ordinary form of conjugality." "'For the wife and kiddies' a man will work fingers to the bone and brain to dullness; will swallow injuries and insult; will lie and steal and cheat his best friend and even commit murder; but he will not risk his life making great discoveries, or his neck breaking world's records, or his reputation stating unpopular truths."

Has she never heard of Palissy, the potter?

PHYSIOLOGIE GYNÉCOLOGIQUE ET MÉDICINE DES FEMMES.
By H. Vignes. *Masson et Cie*
65 francs 6½ x 10; 565 (paper) *Paris*

This is a valuable addition to the literature of the physiology of reproduction. The material is arranged under eight general heads as follows: Primary sex characters; genital function before puberty; puberty and the realization of the sexual type; the adult woman; the menopauses (spontaneous, surgical, radiation); statics of the female genital organs; influence of various hygienic and other environmental influences on the genital functions; the internal secretion of the ovary. The book is extensively documented, and well indexed.

BIOMETRY

STUDIES IN THE HISTORY OF STATISTICAL METHOD. *With Special Reference to Certain Educational Problems.*
By Helen M. Walker.
The Williams & Wilkins Co.
$5.00 6 x 9; viii + 229 *Baltimore*

At this time, when the history of science is assuming more and more importance, Dr. Walker's book is especially welcome. That not all statisticians have an adequate knowledge of the history of their own *Fach* may be suspected from her note on recent discussions as to which of our contemporaries invented the "gross score" formula for the standard deviation, a formula first published by Encke in 1832. As its name implies, the book is not a general history of statistics, the development of official administrative statistics being merely sketched. Nor does it cover the development of all phases of statistical method. Nevertheless, we may be grateful to Dr. Walker for an excellent treatment of the history of some of the more important topics, especially the normal curve and correlation. Besides special bibliographies of memoirs on the probable errors of frequency constants, on measures of correlation other than the product-moment, and on the Theory of Two Factors, there is a bibliography of sources consulted in the preparation of the study and an index.

SCIENTIFIC METHOD. *Its Function in Research and in Education.*
By Truman Lee Kelley.
The Ohio State University Press
$2.50 5 x 7½; 195 *Columbus*

The versatile and fecund Dr. Kelley publishes in this book a series of lectures originally delivered at Ohio State University, and dealing with scientific methodology, scientific ethics, the measurement of intelligence and achievement, curriculum construction, eugenics, and the mental traits of men of science. The last is an especially interesting application of the inductive method to the history of science.

HANDBUCH DER VERERBUNGSWISSENSCHAFT. *Lieferung 8. Band I. Variations-und Erblichkeitsstatistik.*
By Felix Bernstein *Gebrüder Borntraeger*
7.20 marks (Subscription) *Berlin*
14.40 marks (Single copies)
7 x 10⅛; iv + 96 (paper)

A brief exposition of probability and theory of statistics, followed by a more detailed development of the application of statistical methods to genetic problems. A valuable treatment of the subject, well worthy of the attention of geneticists.

PSYCHOLOGY AND BEHAVIOR

THE HISTORY OF PSYCHOLOGY.
By W. B. Pillsbury.
W. W. Norton and Co., Inc.
$3.50 5¾ x 8½; 326 *New York*

Dr. Pillsbury's history is a vivid reminder of how short a time it is since Psychology, under the alias of Mental Philosophy, was the boon companion of Moral Philosophy and other disreputable characters. There are those who suspect that in spite of its conversion and reformation it still retains a taste for doubtful company. Half of the book is devoted to the period before Fechner, a period in which psychology was for the most part subordinated to metaphysics and epistemology, and in which a rather crude in-

trospection was almost the only psychological method. The latter half of the book treats the seventy years since Fechner published his *Elemente der Psychophysik*, the first work to give psychology a definite experimental foundation, and several chapters are given to contemporary movements. To Pillsbury, as to W. H. R. Rivers, the phenomena of war shock are decisive against Freud's doctrine that the neuroses are always sexual in origin. His conclusion is that "psychoanalysis has had the effect of giving a more important place to the psychological attitude towards nervous diseases, and of emphasizing the fact that a patient's old conflicts and present emotional shocks are important factors in the development of neuroses. It stands as a strange episode in the history of psychology but one that has not been without many beneficial by-products."

Dr. Pillsbury is not a professional historian and cannot be fairly held to the standards of the historian. It is amusing, however, to read that Leibniz was "asked to report on the political events at the court of Frederick the Great." In 1716, when Leibniz died, the future Frederick the Great had attained the ripe age of four years. There were, therefore, probably not many political events at his court for Leibniz to report.

THE SOCIAL WORLD OF THE ANTS *Compared with That of Man.* 2 Vols. *By Auguste Forel. Translated by C. K. Ogden.* *Albert and Charles Boni* $15.00 for two volumes. *New York*

5½ x 9; xlv + 551 (Vol. I)
xx + 445 (Vol. II)

These two large volumes represent practically seventy-five years of enthusiastic devotion to the study of this highly organized group of insects. The author commenced his observations early in childhood. When he entered the university at Zurich he had already acquired a very wide knowledge of the behavior and habits of many forms of ants, and with his training as a physician, neurologist and psychologist, he was especially equipped to continue his studies, particularly in the field of sensory and instinctive behavior. There is much pleasure in store for the student and the amateur naturalist in these volumes. So much of the author's remarkable personality has crept into the writing of them that there is to be derived the kind of inspiration which rarely comes except through personal contact.

The work, in the main, is a collection of previously published material. It is divided into five parts, as follows:

I. Genesis; Forms; Anatomy; Classification; Geography; Fossils.
II. Feelings; Physiology; Ants and plants; Guests; Parasites; Nests.
III. Apparatus; Foundation of colonies; Behaviour in the nests; Cattle; Gardens; Parasites.
IV. Alliances and wars; Parabiosis; Lestobiosis; Slavery.
V. Special customs; Epilogue.

Throughout the text there are numerous very excellent figures, and plates, both colored and black and white.

RÉVISION DE LA DOCTRINE DES LOCALISATIONS CÉRÉBRALES. *By R. Brugia.* *Masson et Cie* 24 francs *Paris*

5¾ x 9¼; iii + 195 (paper)

Basing himself on the neurological experiments of Loeb and on the knowledge of cerebral trauma gained during the World War, Professor Brugia proposes to sub-

stitute for the doctrine of cerebral centers the segmentary or metameric theory of the nervous system, which holds that it

results from a colony of units (segments or metameres) in series; that each metamere is provided with a pair of nerves (neuromere), the one (ganglionic) a sensory nerve, leading from a cutaneous zone (dermatomere) or an organ of special sense, the other a motor nerve, ending in a muscular group (myomere); that there are *no centers*, every reaction being integrated by the whole of the segment, and being produced without the specific intervention of cellular elements; that co-ordination is based on the unitary and chain-like structure of the whole neuraxis, on the activity of the fibrils, perhaps uninterrupted and disposed in a closed network like that of the capillaries . . .; that the cell bodies, not inherent but adherent, the fibrils passing through or around them, provide for generic functional activities, that is, contribute to the same conductivity which appertains to the fibers, and moreover serve *probably* . . . as accumulators and distributors of potential energy, and *undoubtedly* as means of nutrition, as is demonstrated by the degeneration of the axis-cylinder when the cells are separated or destroyed.

The book is well worthy of the attention of every neurologist. By the way, was it Augustine or Tertullian who wrote, "*Credo quia absurdum est*"?

THE PROJECTION OF THE ASTRAL BODY.

By Sylvan J. Muldoon and Hereward Carrington. *Rider and Co.*

18 shillings net 5⅞ x 9; 242 *London*

Mr. Muldoon, who has been a projector for twelve years, gives detailed directions in this book for projecting one's astral body. However, the reviewer regrets to report that in his own case following these directions has not yet produced results. There is an interesting passage on occult dietetics, quoted from an article by Prescott F. Hall in the *Journal of the American Society of Psychical Research:*

A vegetable diet tends to loosen the vibric matter of the astral body; and vegetables, fruits, and prunes make the blood able to attract spiritual power. Carrots also are beneficial. Nuts, especially peanuts, are bad—especially near the time of sitting for development, as they tend to make one's atmosphere of one colour. Raw eggs are favourable. Liquids are favourable to development . . . . Fasting often helps the liberation of the astral body.

Now we know why the inhabitants of boarding houses are so psychic. It is a result of their diet of prunes.

THE WAY WE THINK. *A Primer of Education and Psychotherapy by Reëducation. By H. Travers Cole.*

Occult Publishing Co.

$2.00 (Regular Edition) *Chicago*

$3.00 (De Luxe Edition)

5½ x 8½; 171

A contribution to psychology from the realm of learning where osteopathy is medicine. It is taught that the rhythmic pulsation of the brain discovered by Vittoz is the physical sign of sufficiency of brain control. The rate is usually half the number of heart beats and twice the number of lung movements, and mounts to more rapid rhythms as thinking energizes the mechanisms of sensation, idea and act. By feeling in on the brain the unprejudiced physician can detect the irregular pulsation of the psychasthenic and has within his grasp the technique to effect prevention and cure.

SELECTIVE TRANSPORTATION BY CHIMPANZEES. *Comparative Psychology Monographs, Vol. 5, No. 4, Serial No. 26. By Harold C. Bingham.*

The Johns Hopkins Press

75 cents 6¾ x 10; 45 (paper) *Baltimore*

A report of a series of interesting tests on four chimpanzees who were put to work to solve the problem of moving food

from an inaccessible place within a wire cage to an opening where they could easily procure it. Food was suspended in the cage in such a way that it could be moved, by manipulation from the outside, to one of four openings near the base of the cage. Each animal was allowed ten minutes to find the solution. All failed the first time. Three succeeded in the second trial, while the fourth needed one more chance. A number of variations were later introduced to make the solution more complicated. The author believes that one of the most important contributions of these experiments is the "opportunity of seeing accidental, habitual and ideational behavior in immediate contrast." Space does not permit a detailed report of the results. The general conclusion which he reaches is that the behavior of the chimpanzees exhibits adaptive characteristics which are ideational.

THE PROCESS OF HUMAN BEHAVIOR.
By Mandel Sherman and Irene Case Sherman.
W. W. Norton and Co.
$3.00 5½ x 8¼; 227 *New York*

This book is the result of six years of study of the development of behavior in human infants. Direct observations were augmented by motion pictures. The book opens with two introductory chapters on the growth and significance of the nervous system. Then follows the discussion of the authors' investigations under the following headings: The first human responses; sensori-motor development; observation of the emotions; nature of the emotions; development of personality, and personality and social behavior. The authors present much interesting and original material. They have wisely excluded discussions of the various schools of psychology. The final paragraph in the book briefly summarizes the conclusion which is reached by this study:

> We have seen how adaptive behavior develops as a result of experience. Personality, then, which is made up of characteristic behavior, is especially fashioned by experience, for the process of human behavior is truly a developmental process of adaptation of the individual to his environment.

THE SCIENCE OF PSYCHOLOGY. *An Introductory Study.*
By Raymond H. Wheeler.
Thomas Y. Crowell Co.
$3.75 5½ x 8½; xvii + 556 *New York*

An excellent general textbook of psychology. The chapter headings will indicate the plan of organization of the material. After an introductory chapter dealing with general definition, and a brief historical review, there follow chapters on: social behavior (two chapters); intelligent behavior (two chapters); emotive behavior (three chapters); learning behavior (three chapters); simple reaction, and observational behavior (two chapters); general summary. The point of view throughout is the organismal-configurationist. The book is well documented throughout, and carries detailed author and subject indices. It is a notable addition to the textbook literature of the subject.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

HANNO *or The Future of Exploration. To-day and To-morrow Series.*
By J. Leslie Mitchell.
E. P. Dutton and Co.
$1.00 4⅛ x 6; 94 *New York*

The second chapter of this book is a sane and interesting discussion of the

future of exploration in the Arctic and Antarctic, the Sahara, Arabia and other *terrae incognitae*. With Byrd's flight to the South Pole fresh in mind it is intriguing to come on Mr. Mitchell's conclusion that

> for purposes of northern exploration the aeroplane has been much over-praised. It is no more an instrument of exploration than Herr Amundsen is an explorer. The trans-Arctic flights early in the present decade achieved nothing; they were mere exuberances of ignorance favoured by inexuberance of climate. As a scouting and general survey instrument it has its uses under favourable conditions.

In the later chapters Mr. Mitchell's imagination, taking the bit in its teeth, reaches a goal much like one of the earlier novels of H. G. Wells. Not only will men penetrate the earth crust to the subterranean seas and the great cavern which, he believes, occupies the interior; they will traverse the interplanetary spaces and explore the craters of the moon and the canals of Mars.

HERACLITUS *or The Future of Films. To-day and To-morrow Series.*
By Ernest Betts. *E. P. Dutton and Co.*
$1.00 4⅛ x 6; 96 *New York*

This is not so much the author's projection of what the films are likely or inevitably to be as what he would like to see them be. It is an essay on the possibilities of the movies as a fine art as esoteric as that of Michel Angelo. Mostly it is a plea for the free play of the creative spirit in the cinema as opposed to the present condition in which a hundred influences emanating from the box office dominate it. The author would simplify the movie theater itself and experiment with such things as the musical accompaniment and the introduction of sound. He envisages the movies of the future as pictorial poetry.

The book is done in a bouncing, entertaining style and contains one *mot* worth quoting. Israel Zangwill is reported as saying that the whole movie is more important than the actress' part and the author adds, "*Sic transit Gloria Swanson.*" [NOTE: Through the kindness of the well-known movie magnate, Mr. J. Pierpont Cohenheimer, the QUARTERLY REVIEW hopes to be able to offer a prize of $100,000 (in Confederate money) for the best essay of not less than 5000 words stating *why* this was worth quoting.]

SCIENCE IN LITERATURE. *A Collection of Literary Scientific Essays. Edited by Frederick H. Law.*
Harper and Bros.
$1.20 5 x 7½; xvii + 364 *New York*

A collection of twenty literary, scientific essays to provide suitable material for College Entrance Requirements in English. A few of the titles, selected at random, are: Secret of the charm of flowers, by William Henry Hudson; Secrets of the ocean, by William Beebe; On a piece of chalk, by Thomas Henry Huxley; The discovery of radium, by Madame Marie Sklodowska Curie; The atomic theory, by Edward Neville da Costa Andrade; Electricity and civilization, by Charles Proteus Steinmetz; Pivotal figures of science, by Arthur Elmore Bostwick.

ATALANTA *or The Future of Sport. To-day and To-morrow Series.*
By G. S. Sandilands. *E. P. Dutton and Co.*
$1.00 4⅛ x 6; 117 *New York*

To get a running start for the future of sport Mr. Sandilands devotes the greater part of his book—and to our notion the most interesting part—to the past and present of sport. We recommend to all lovers of the pugilistic art the account of the famous match between Tom Sayers and the Benicia Boy. There were giants in those days!

MRS. FISHER *or The Future of Humour. To-day and To-morrow Series.* *By Robert Graves.* *E. P. Dutton and Co.* $1.00 4¼ x 6; 95 *New York*

If the humor of the future is as dull as *The Future of Humour* God pity the people of the future!

EDITORIAL NOTE

It has always been a deplorable fact that American biologists have known relatively little about the productive activities of their Russian colleagues. This is, of course, because of the language barrier. By the election to the Editorial Board of THE QUARTERLY REVIEW OF BIOLOGY of W. W. Alpatov, *Privatdocent* in the University of Moscow, and well known for his numerous publications in the fields of entomology, biometry and experimental zoology, it is hoped that the QUARTERLY REVIEW may contribute something towards a better interchange of knowledge between Russian and American biologists.